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CLINICAL LECTURES,

DELIVERED AT UNIVERSITY COLLEGE HOSPITAL,

BY

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PULMONARY CONSUMPTION.

GENTLEMEN:—I am desirous, to-day, of directing your attention to two cases of phthisis lately admitted to the male ward, both of which are highly deserving of your attention, on account of the differences which they present in regard to the probable origin of the disease, its progress, and the morbid phenomena by which its presence is recognised in each.

The first patient, James Shirley, 36 years of age, of a stout, muscular form, states that he never had a day's illness before the present attack, which was only two months and a half ago, although the disease has already arrived at the commencement of the second period; that is to say, the softening of the tuberculous matter, and the formation of tubercular excavations.

The second patient, William Calvert, aged 40, of a slight habit of body, and nervous temperament, dates the commencement of the disease to a somewhat later date; yet, in this patient, it may be said to be only at the commencement of the first stage; that is to say, the material or essential anatomical element of phthisis, the tuberculous matter, is yet in that state which is generally denominated the state of crudity. Under these circumstances, and as the physical signs of phthisis pulmonalis are derivable from the presence of the tuberculous matter, and the morbid conditions to which, as a foreign body, it gives rise in the pulmonary tissue, it is obvious that these signs must, in these two cases, be considerably modified; and so much so is this the case, that whilst in the first patient, in whom the disease is advanced and progressing rapidly, the physical signs are marked and readily detected by percussion and auscultation; in the second these are so slightly announced by these means that an ordinary observer might overlook them altogether. The existence of phthisis in this patient is, however, not less certain than in the former,—as certain, indeed, as if we heard those sounds which indicate the presence of tubercular excavations. I shall presently state the grounds on which the certainty of our diagnosis is founded in this and similar cases. In the meantime I shall briefly state the case of William Calvert, the patient in whom we have the example of incipient phthisis.

This patient, 40 years of age, was admitted the 13th of this month. He is a tailor by profession; rather of regular habits; both of his parents died of some affection of the chest; he has brothers and sisters who are all healthy; he himself has always been delicate. At the age of 18 he became subject to colds; fourteen years ago he had rheumatic gout; when a boy he had frequent attacks of epistaxis. *Three* months ago, having caught a fresh cold from coming out of a warm workshop into the cold night air, he became subject to a cough, more severe than any preceding one, and which was accompanied by occasional hæmoptysis. The blood spit up was usually in streaks in the sputa, but sometimes in clots. The hæmoptysis, when it first appeared, continued for two days, but was not very profuse. It has gradually diminished in quantity, and became fainter in colour since the commencement of the attack.

On his admission he presented the following symptoms:—Pain and constriction across the chest; headach; considerable heat of skin; profuse perspirations at night; alternations of heat and cold; occasional rigors; weakness and pains in the limbs, especially in the knees, which are worse when he is hot. On percussion the sound is *dull* on the right side, under the clavicle; in the same situation, on the opposite side, it is natural. On the same side, viz. the right, on which the sound is dull on percussion, the respiratory murmur is *diminished in intensity*, and is somewhat *rough* or *harsh*. This character of the respiratory sound accompanies not only *inspiration*, but also *expiration*; and,

besides, the expiratory sound is greatly *increased in intensity*, being nearly equal to that which accompanies inspiration. On the left side, that on which the sound on percussion is clear, the respiratory sound is, perhaps, stronger than natural; *expiration*, however, not being accompanied by a proportionate increase in intensity of the sound to which it gives rise; the breathing is short; pulse 76; appetite bad; bowels confined; flatulency; urine plentiful, but at times high coloured.

Taking a general view of this case, we find in it a good illustration of the manner in which the phenomena of phthisis pulmonalis are successively developed, more especially where there is an hereditary predisposition to the disease, as was probably the case in our patient, whose parents are said to have died of a chest affection, and notwithstanding that his brothers and sisters are stated to be in good health. From his boyhood upwards he has been delicate and subject to colds, and has not got rid of the cough with which he became affected three months ago. This last cough has been accompanied by hæmoptysis, the blood appearing in the sputa, either in streaks or clots. This has gradually diminished, and ultimately disappeared: but it has been succeeded by various constitutional derangements, chiefly febrile symptoms of a hectic character, viz. alternations of heat and cold, occasional rigors, and profuse perspirations. Of these precursory symptoms by far the greatest amount of value, in a diagnostic point of view, is to be attached to the hæmoptysis, as predicating the existence of tubercular phthisis. Its occurrence in this case along with, and after repeated attacks of cough, increases its diagnostic value; and taken in connection with the local signs which I have enumerated as existing in the right side of the chest, we cannot hesitate to attribute its occurrence to the presence of tubercles in the lungs; as indicating, in fact, three months ago, the existence of tubercular phthisis in this patient. In attaching so much importance to hæmoptysis I do not mean to convey the idea that it may not frequently occur, and to a great extent, independently of the presence of tubercles in the lungs. On the contrary, it is well known that hæmoptysis may be the consequence of disease of the heart or large blood-vessels, and more especially the consequence of deranged menstruation, and other obvious morbid states; but when none of these causes are present, and when it occurs repeatedly, either before or after the supervention of cough, it is of all the symptoms at the commencement or early stage of phthisis, by far the most important as indicating the existence of the disease. Of the value of this symptom of phthisis you will consult with advantage the work of M. Louis, whose labours on the pathology of this disease will afford you much practical diagnostic information.

I may, however, further observe, in regard to this important symptom, that although it most frequently occurs in patients who have, as in the case of our patient, been subject for a variable length of time to cough, it is not unfrequently met with in others who have never had cough; who are, in fact, to all appearance in the enjoyment of perfect health; and in such cases, and in the absence of other causes, is the first symptom of incipient phthisis, or of the presence of tubercles in the lungs; and I may further notice the fact that hæmoptysis is a very rare occurrence, indeed, in catarrh or bronchitis, a fact which gives additional value to its presence as a diagnostic symptom of phthisis.

I have said that the local signs furnished by percussion and auscultation in this case, greatly increased the diagnostic value of the hæmoptysis as a symptom of the early stage of tubercular phthisis. These local signs were limited to the right side of the chest, within a circumscribed space under the clavicle. They consisted of some dulness on percussion; a rough or harsh sound during inspiration; and of a sound nearly as strong, and much of the same character, during expiration. All of you are, no doubt, familiar with the value of the first sign, or dulness on percussion in the subclavicular region, as indicating the existence of tubercles in this region of the chest, this being so frequently the primary seat of these bodies as to constitute a law in regard to their relative frequency in the pulmonary organs. Here, however, the dulness was so slight that it might have been overlooked, or its importance might have been underrated, but for the harshness of the sound which accompanied inspiration, and more especially the existence of an analogous sound during expiration. You are aware that in the healthy state of respiration the sounds which accompany inspiration are the vesicular, bronchial, and tracheal; that those which accompany expiration are the bronchial and tracheal. There is no vesicular murmur heard, and even very little of the bronchial sound, during healthy expiration. The reason why the sounds alluded to are produced with greater intensity, and in greater number, in inspiration than in expiration, is sufficiently obvious if we reflect on the mechanism of respiration. During inspiration the air, as has been remarked by Dr. Williams, is the moving body, and entering the lungs with considerable velocity, impinges against the angles and sides of the bronchi and cells which it has to dilate, and must give rise to sound throughout the whole course of its passage. During expiration the air, on the contrary, is put in motion by the compressed and contracting lungs, and yielding passively to this cause, does not acquire motion or resistance enough to produce sound, until, by the con-

verging together of the small tubes, it is gathered into a current in the larger tubes, where, impinging against their sides with its now acquired velocity, it at length produces sound. Hence you can perceive why sound is heard only towards the middle or termination of the act of expiration, and heard only in bronchi of a certain size; and why it should possess a somewhat hollow and blowing character, rather than the diffuse, soft, vesicular murmur of inspiration, heard over the greater part of the surface of the chest.

I have mentioned these circumstances in regard to the production of sound during inspiration and expiration, that you might perceive more clearly the value of the sign observed in this patient manifested during the latter act. Instead of being feeble, the sound of expiration is increased, and nearly as loud as that of inspiration. Now, what is the cause of this, and how is it occasioned? That it depends on the presence of tubercle there seems to be now no longer the slightest doubt, Louis, Andral, and others having verified the fact since it was first pointed out by the late Dr. Jackson, a young American student, while studying the stethoscopic signs of phthisis in Paris.

As to the manner in which this sound is produced, or becomes perceptible to the auscultator, the following is the explanation given of it by Dr. Jackson:—

"As soon as tuberculous matter is deposited there exists a solid material around the bronchi, which will transmit the sound made by the passage of the air through these tubes. But at this early period of the disease a certain portion of the lung in the part affected is still permeable to the air, and, therefore, the murmur of vesicular expansion, during inspiration, entirely masks the sound of the air passing through the bronchi, which would otherwise have been transmitted through the denser surrounding medium. During expiration, however, circumstances have changed; the air on passing through the bronchi produces the same sound as on its entrance; and, as now there is no vesicular expansion to mask it, it is easily transmitted through the diseased or condensed part to the ear of the observer."

How far this explanation of the sound in question be the correct one, is a matter of no great importance; its occurrence, under the circumstances which I have mentioned, at an early stage of phthisis pulmonalis, and as indicating the presence of tubercles in the portion of the lung in which it is heard, is certainly one of the most important of the physical signs of this disease.

Before leaving this case, I may direct your attention to the sputa, which are peculiarly characteristic of the early stage of phthisis, when not complicated with bronchitis or pneumonia. In this patient they form a striking contrast to those observed in the other patient, in whom the disease is much farther advanced. They consist of a grey or pearly-coloured substance, of the consistence of tough mucus or boiled albumen, semi-transparent, collected into rather small irregular masses, swimming in a moderate quantity of a clear watery-looking fluid, or slightly adherent to the vessel in which they are contained. The quantity of the sputa may vary considerably, but in general is not great, and in our patient is small, and is coughed up with considerable difficulty.

These, then, are the principal circumstances to which I have been desirous of directing your attention in this case, viz., the characters of the sputa, the physical signs detected by percussion and auscultation, as signs of the early or incipient stage of pulmonary phthisis, and the hæmoptysis by which it is so frequently preceded.

I shall now make a few remarks on the principle of the treatment which I have adopted in this case; and I may first observe that the case is one which, as regards the stage of the disease, and its limited extent, offers a fair chance of success to any plan of treatment which has received the approbation and recommendation of practical physicians. The plan of treatment to which I now shall allude is that which consists chiefly in the frequent use of *emetics*. Whatever may have been the theory of the disease which suggested this plan of treatment, it was, at a remote period, adopted and recommended by several eminent physicians, as the most successful; and numerous cures of phthisis are reported as having been accomplished under its judicious management. It is a method, however, which was never generally adopted, and must, no doubt, have often sadly disappointed both the patient and the practitioner; and, besides, when we reflect on the very imperfect means which the physician, at the time this practice was most in vogue, possessed of determining the existence of phthisis pulmonalis, or of discriminating between this so frequently fatal disease, and other usually curable diseases of the chest, with which he must frequently have confounded it, we cannot place much reliance on the curative effects of the emetic plan of treatment under such circumstances. No modern physician, qualified for the task, that is to say, capable of establishing the diagnosis of tubercular phthisis by means of its *physical signs*, has, as yet, so far as I am aware, given this plan of treatment a fair trial, or furnished us with facts deserving of the slightest confidence.

Only seven years ago, a modern physician, certainly, published a small treatise, entitled "Observations and Experiments on Phthisis Pulmonalis, followed by a particular Method of Cure of this Disease." The author of this treatise is a Neapolitan, Gio-

vanni de Vitis, first physician of the Military Hospital; and when I first heard of the number of cases of cure of phthisis which he states to have accomplished, my feeling of disbelief was so strongly excited that I gave but a very limited credence to the statement, and this I was the more disposed to do, that I had learned from personal observation the incapacity of physicians in the Neapolitan and Roman States of discriminating between phthisis and some other curable affections of the chest, more especially chronic catarrh and its complications, and chronic pleurisy. It is only within these few days that I have been able to procure the work of this author, and the perusal of it has more than confirmed the statement I have just made as to the utter worthlessness of his authority on such a subject, and the results which he has published of his new method of treatment. He does not even seem to know that such means as percussion and auscultation are employed to detect the existence of the disease; and yet, according to the old symptomatological system, he boldly classes the cases he has submitted to his method of treatment, under the heads—First, of chronic catarrh; second, of phthisis in the first degree; third, of phthisis in the second degree; fourth, of phthisis in the third degree. Of these he says he has cured forty cases of chronic catarrh; forty-seven of phthisis in the first degree; one hundred and two of phthisis in the second degree; and twenty-seven of phthisis in the third degree.

I shall not detain you by detailing the symptoms of any of the cases he has published, or the length of time which the treatment lasted, and for the reason I have already stated, that no confidence can be placed in statements regarding the cure of a disease such as phthisis, unless we possess the most satisfactory evidence in the first place of the existence of the disease before the treatment, and of its non-existence at some subsequent period. However, it is on other grounds than those I have already alluded to, that I am disposed to attach some importance to the treatment of phthisis by emetics. If the result of my researches on the seat and nature of tubercle is founded on fact, it affords some grounds for the rational hope that the cure of pulmonary phthisis may be promoted or facilitated by the employment of emetics. If, as I have endeavoured to prove, the tuberculous matter which constitutes the material cause of the disease, is contained principally, or in the great majority of cases, within the air-cells and minute bronchi, it is easy to perceive that its expulsion will be effected or promoted by the employment of such means; that the destruction of the pulmonary tissue will be less likely to occur, or occur less extensively; and that time may be afforded for the correction or the removal of that state of the constitution on which the formation of the tuberculous matter essentially depends, and without which the cure of the disease would in vain be attempted. The employment of emetics, under circumstances so unfavourable as those in which patients are placed in hospitals, will, I am afraid, be attended by disappointment, as, whatever efficacy they may possess in effecting the dislodgment and expulsion of the tuberculous matter, we cannot, at the same time, obtain that assistance from other means derivable from the well-regulated influence of temperature or climate, including pure air, exercise, and a variety of hygienic conditions, which conjointly contribute to the same end, and more especially towards the removal of the tubercular diathesis.

As, however, the case which has given occasion to these remarks presents a favourable opportunity for trying the efficacy of emetics, half a grain of the tartarised antimony, in solution, has been ordered to be taken every morning, and to be repeated, if necessary, until vomiting has been produced. The bowels to be regulated by the occasional use of a calomel and colocynth pill; and for the present the patient is to be confined to low diet. The result of the treatment will be made known to you, and I would request you to observe for yourselves, and to examine the chest of the patient with care, that you may fully appreciate the importance of the physical signs which announce in him the existence of phthisis.

Having dwelt so long on this subject, I shall be brief with the observations I have to offer you on the second. I have already stated that this patient, James Shirley, æt. 36, of a stout, muscular form, never had a day's illness before the present attack, about two months and a half before his admission into the hospital, the 30th of this month; and yet how different the state of this patient from that of William Calvert, whose case I have laid before you, in whom the commencement of the disease dates only about two weeks earlier. In this patient it has, as I have stated, arrived at the second stage; the extent of the tubercular deposit is much greater, exists on both sides of the chest; but on the right side, the same side as in the other case, the signs of softening of the tuberculous matter and of tubercular excavations, were recognisable more than two weeks ago, at the time he was admitted into the hospital.

The history of this case is very similar to that of the other. The patient caught cold, and soon became subject to a slight cough, which gradually increased; and, in the early part of Christmas, was followed, for the first time, and after a fit of severe coughing, by the discharge of about a teaspoonful of blood. A week after he spit about the same quantity of blood, after coughing. On the 2nd of January, after a paroxysm of

coughing, he brought up nearly a pint of blood, and, two days after, to the amount of two pints. After this period he spat up, on two separate occasions, small quantities only of blood. Having gone for advice to a dispensary, he had a few leeches given him to be applied to the chest, and, some days after, was cupped on the same part, apparently without any relief to the cough, which is said to have gradually increased, whilst the expectoration, at the same time, became more profuse.

On admission the cough was very troublesome; worse at night, as is generally the case; it was accompanied by the expectoration of a considerable quantity of a thick, greenish-yellow coloured, rather tenacious mucus, somewhat frothy, and sometimes tinged with blood, and brought up with difficulty. On examination, sonorous, sibilant, and mucous rattles were heard in various parts of the chest. Besides, there was heard a slight gurgling sound under the right clavicle, dulness on percussion on the posterior part of the chest, opposite and over the clavicle, and occasionally a cavernous ring on coughing. The sounds of the heart were normal; pulse 110; skin hot and dry during the day, but perspiring during the night, and sometimes profusely; tongue whitish and slightly furred; throat sore, from constant coughing; appetite good; no sickness; bowels open from medicine; urine less than in health, and deposits a sediment.

I shall merely observe that in these symptoms you have, first, the signs of bronchitis, and, secondly, those of phthisis. The yellowish-green coloured expectoration; mucous, sonorous, and sibilant rattles, indicated the presence of the bronchitis, and that of a subacute character, and which appears to have existed for some time. The dulness on percussion, the gurgling sound, and cavernous ring, are, in this case, equally characteristic of the presence of tubercular phthisis, not merely because of the existence of these signs, but because of their being heard at the summit of the upper lobe of the lung, and for the reasons with which you are already acquainted.

This patient had already lost much in strength and flesh; and that roughness of the voice so common in confirmed phthisis was also very conspicuous, and has of late become more so. He has been three weeks in the hospital, and has obviously become weaker and leaner; the cough remains much the same; expectoration is somewhat more easy; the sputa, although perhaps not so great in quantity, has more of the puriform character, is sometimes thick and adhesive, indicating the persistence of the subacute bronchitis, and are still tinged with blood; and the night perspirations are more profuse.

On the admission of the patient cupping between the shoulders was employed, and was followed by some relief to the cough; and, during the first week, he took an expectorant and anodyne mixture during the day, and an opiate at night. During the second week he was put upon the use of the solution of tartarised antimony, to be taken every morning. This was followed each time by vomiting, and the expectoration of a considerable quantity of muco-puriform matter. As, however, no amendment appeared to take place under the use of this emetic solution, and as the patient complained of great fatigue from its operation, and also of weakness, it was laid aside.

I cannot conclude without remarking that there is little hope of being able to do any permanent good in this case. If the disease has not advanced since the admission of the patient, it has exhibited no symptom, as I have already said, of amendment. The extent of the hæmoptysis in the first period of the disease is generally an index of the extent of the tubercular deposition, and as it was very considerable in this case, the more unfavourable is the prognosis, independently of the other circumstances which I have named as leading to the same conclusion.

ABSTRACT OF

A CLINICAL LECTURE

ON

DISEASES OF THE SPINE,

BY FREDERICK HALE THOMSON, ESQ.,

Assistant-Surgeon of the Westminster Hospital.

DELIVERED ON MONDAY, APRIL 29, 1839.

GENTLEMEN:—In my two preceding lectures I spoke to you of those distortions which result from disease in the bones, in the intervertebral substance, or in the ligaments of the spinal column. Each of these textures we find is as liable to inflammation, and its train of consequences, as any of the softer textures of the body. The same series of changes take place in both kinds of structure; in the soft and in the hard parts. When a solution of continuity occurs in the substance of the hard parts, they are susceptible of restoration by the same reparative process which is found potential in supplying the deficiencies of every other tissue in the frame. But the reparative process in diseases of the vertebral pile is slower in its operation than in the softer parts of the body. If the surgeon should lose sight of this fact, while devising his plan of treatment, considerable deformity may arise, or perhaps some more serious mischief may be occasioned,

Absorption of the bodies of the vertebræ, which I have described in my former lectures as taking place in thirty-four cases out of three hundred, is not necessarily attended with deformity. The tilting of the spinous processes, which is seen in the majority of cases, is caused by the absorption of the anterior part of the bodies of the vertebræ, whilst the posterior retain very nearly their natural bulk. I have seen cases where the bodies of the vertebræ have been extensively absorbed without producing curvature in any direction. The case of the woman Ridd, now in the hospital, exemplifies this fact; she has merely a curtation of the spine, as you will see upon examination. As I have stated that posterior curvature, attended with disease, is much more frequent than the same deformity unattended with morbid change, an exceedingly interesting question arises in the mind, whether the deformity, or projection called "hunchback," which results from this state of parts, can in any instance be removed, or materially obviated? From the experience I have had, I have no hesitation in saying that in many cases this deformity may be safely and completely remedied. The late Mr. Shaw,—the excellence of whose book on diseases of the spine I have already had occasion to commend,—denied the practicability of any such curative process. Mr. Shaw's animadversions, however, were chiefly directed against a contemporary practitioner, who roundly and habitually asserted that, by certain manipulations, and the application of mechanical force, he could replace the dislocated bones in their normal situation. To Mr. Shaw's zeal in the cause of anatomy, and his ardent desire to refute the empirical dogmata of his opponent, much of the force of his language is to be ascribed, and there is every reason to believe that had he lived to the present day, his excellent understanding would have prompted him to abandon, as untenable, many of the positions taken by him. Nothing could, indeed, be more preposterous than the assertion, that a long-established dislocated vertebra could be suddenly and forcibly pushed into its proper place; and we must agree with Mr. Shaw, that it was nonsense to assert that, by any combination of thumbing or handling, it was possible to disunite the brittle, unelastic, and anchylosed vertebræ, and place them in their pristine bulk and position, and thus restore the patient to health, strength, and symmetry.

Mr. Shaw was an accurate anatomist, and well acquainted with surgical pathology; and from the contemplation of the human frame in the dead subject, as well as from a laborious study of pathological preparations, he deduced the principles which formed the basis of his therapeutical system. But who, from the contemplation of the ordinary framework of the human body, would, *a priori*, infer the possibility of the feats which are performed by certain posture-masters, rope-dancers, and tumblers? These men continually perform movements of the body the attempt of which would be death to an ordinary individual. Who, from the natural structure of the spine, could infer the flexibility which it occasionally exhibits in these mountebanks? I have myself seen an Italian posture-master (Mazurri) whilst standing on his feet, bend back until his head touched the ground, thus causing his spine to form a complete bow. By excluding this important class of facts from his consideration, Mr. Shaw was led to reject certain principles of treatment which a more extensive induction would have made evident to his mind. He ought to have studied the human frame in action as well as at rest, and he would have arrived at the true principles of therapeia in these maladies. I have no doubt that if Mr. Shaw had been spared to us, he would have altered his opinion as to the impossibility of relieving distortion of the spine even after caries and absorption have taken place.

The Reverend Mr. Wass, in the 33rd vol. of the "Philosophical Transactions," has recorded some very interesting observations touching the condition of the vertebral column. He states that the height of people under forty years of age is diminished to the extent of an inch during the day, each person being shorter at night than on rising in the morning. But as this curtation might, to a certain extent, be ascribed to the joints, the results, as regard the condition of the spinal column, were a little varied. He observed that the shortening was less in labourers, and persons accustomed to out-of-door exercises, than in sedentary men. In the same volume of the "Transactions" Mr. Beckett also published a series of observations confirming the statements of Mr. Wass, but conducted so as to exclude a source of error admitted into his calculations. The measurement was taken of those who had been seated all day, and thus the influence of the contraction of the joints was obviated. In children this decurtation of the spine was found to be much greater than in adults. These are facts which demonstrate the great pliability of the spine, and the influence which the mere weight of the body exercises upon it. Cannot this force be made available for a therapeutical purpose? Decidedly it may; and it forms a more eligible and manageable power than that "pressure from without" which the empirical proprietors of backboards, spine-supports, &c., so clamorously recommend. I have already stated that the incumbent weight of the head and trunk on the spine constitutes a great source of evil to the afflicted patient, and that the removal of this pressure has consequently formed a

leading principle in all the systems devised for the cure.

In fact, two principles have been adopted in the treatment of these diseases:—1. The removal of the weight of the head and shoulders from the spine; and 2. The adoption of means to ensure the elongation of it. Various have been the mechanical contrivances invented for these purposes: chairs have been constructed with crutches to support the axilla, but so high that the patient's pelvis could not rest upon the seat. By their means the superincumbent weight of the head and shoulders is removed from the spine, and the weight of the pelvis and lower extremities exercises an elongating force upon it. Graduated spine-supports of various forms are in constant use for the same purpose. Apparatus analogous to these have been in constant requisition since the days of Ambrose Paré, and have been the means of enriching thousands of pretenders, and confirming the deformity of many persons whom the unassisted and unrestrained efforts of nature would have restored to symmetry. Stretchers of various constructions have been employed from an early date, and are at the present day extensively used in France and Germany. The inventors and employers of the complicated machinery alluded to, aim at effects which are perfectly legitimate (provided that the means employed are consonant to the dictates of physiology and the principles of pathology), namely, the removal of all weight on the spine, and the extension of the curved column.

When the active inflammation (which is the source of the mischief in the affected structures) is reduced, the patient may be placed on a slightly inclined plane, on which should be two elevations designed to arrest, at the axilla, the pressure of the head and shoulders. The lower limbs and pelvis, by their weight, exercise a constant and gentle traction on the spine, and very gradual pressure may be applied to the convexity of the curved column by interposing a hair pillow between the "hunch" and the inclined surface on which the patient lies. This treatment continued for a sufficiently long period will, in the majority of instances, prove highly beneficial.

There are many reasons which should prompt the medical man to undertake the removal of curvature of the spine, principally because it induces derangement of the functions in many of the most important viscera, and often occasions contraction of the thorax, consequently diminishing the area intended by Nature for the play of the lungs, heart, and the larger chylopoietic and assistant chylopoietic viscera; therefore, by removing spinal malposition, the cause of many disorders is obviated.

all ranks of society, but especially in the higher circles, a morbid sensibility characterises persons afflicted with deformity; they fancy themselves objects of ridicule, and therefore their happiness is, in a great degree, impaired. These are considerations that naturally stimulate the philanthropic medical man to persevere in his endeavours to eradicate the sources of this misery, or at least to alleviate it.

You will generally find, Gentlemen, that the patients who are brought to you for relief in these diseases are loaded with irons, straps, and buckles, springs, bands, and similar accoutrements. You will, of course, order their immediate removal; but you will find that obedience to your mandate is not given without reluctance, and some firmness is necessary in insisting upon the execution of it. People are so prepossessed with an idea of the great efficacy of machinery, that they cannot at once divest their thoughts of the prejudice in its favour. Sometime since a lady brought to me her little boy, who was affected with this antero-posterior curvature, and whose lower limbs were also distorted. He had been made to wear irons on the legs. According to my custom I ordered their immediate discontinuance, but I had great difficulty in gaining the consent of the mother, and persuading her to adopt my plan of treatment. The process which I invariably recommend is the following, and for several years I have never met with a single instance in which it has failed:—I place the patient on a high chair, or stool, in the sitting posture, and cause them to flex and extend, alternately, the legs upon the thighs, until some power is evinced in the muscles of these parts. After this I cause the patients to stand behind a bar, or a chair, and to endeavour to raise themselves on their toes; this they will find impracticable until after a great many trials. For the first three weeks they will fail almost entirely, but you must not be discouraged, nor suffer them to be disheartened. In process of time this plan will inevitably succeed; the muscles will acquire tone, and the bones a normal consistence.

I have repeatedly remarked, that when the antero-posterior curvature occurs at an early age it is a constant sign of the presence of scrofulous disease. This diathesis seldom limits its influence to one organ or one part of the osseous system; it is obvious, then, that distorted spine and bent limbs are often concurrent, as in the case just alluded to. Before the myological treatment is adopted care must be taken to ascertain the perfect cessation of the inflammatory action; the vital processes which we avail ourselves of in the treatment of these maladies are exceedingly slow in their operation, and time is, therefore, required to develop results. Patience and constancy are indispensable in a course of treatment.

A recent case that came under my care affords a striking exemplification of the

truth of this statement. A boy, scarcely nine years of age, had for a long time been under the care of several practitioners, regular as well as empirical; he had experienced no benefit, and his life was despaired of. He was very scrofulous, the antero-posterior curve existed, and considerable absorption of the bodies of the dorsal vertebræ had occurred. He was placed by me on a gently inclined plane, and the pressure of the head and shoulders was removed by prominences placed under the axillæ. The weight of the lower limbs exercised a gentle traction upon the curved column; a hair pillow was placed between the spine and the supporting plane, and in the course of about six months the patient was in good health, and his locomotive functions well restored. He can now run and dance like other children.

I have stated that posterior curvature is much more common in the children of the poor than in the children of the rich. This disease is a result of mollities ossium and of a scrofulous diathesis, which may be produced by the great exposure to physical evils inseparable from the condition of the former class. It is remarkable that lateral curvature is very rare amongst the poor; in five years I have not met with more than seven cases. When they do occur in that class, it is amongst nursery-maids and maids of all-work, who have either to nurse heavy children, which occasions a curvature towards the side on which the child is habitually carried, or who, having work to perform disproportioned to their strength, acquire certain habitual motions, which employ particular muscles, and thus gradually distort the column of the vertebræ. In the lower classes distortion in the antero-posterior direction is as common amongst male as female children, a circumstance which distinguishes the cases occurring in these classes from those in the upper circles.

Incurvation seems to be equally common in all ranks; it is in many instances the result of paralysis in early life; the muscles immediately acting on that part of the spine are atrophied, and the vertebræ deriving no lateral support from the ribs, are more liable to be displaced by the operation of accidental forces. These tend to produce distortion in that direction in which least resistance is offered,—that is, forward. The great cause of these maladies in children is gastric irritation; this is often neglected, and exhibits its baneful influence in various ways. The whole cerebro-spinal system is disordered, the secretions are vitiated, and the regular processes of development are interrupted. The disorders exhibited during dentition are examples of this gastric irritation obstructing the natural process of teething, which itself cannot be considered as a cause of the symptoms which are manifested. We find early cases of spinal deformity in scrofulous constitutions accompanied by fits, or by paralysis of one or both limbs; and we may safely infer the existence of a hollowness in the loins, or other twisting of the vertebral pile, when the above-stated symptoms are observed.

In many cases where the lower limbs *drag*, and the muscles are greatly atrophied, the upper limbs are endowed with strength,—a sure indication of the propriety of the myological treatment. The atrophied limbs should first be assiduously rubbed; afterwards the patient should be required, while sitting on a high chair, to swing them backwards and forwards; and, finally, he should be made to exercise the muscles in maintaining and varying the vertical position. This sort of graduated exercise will slowly, but surely, restore the power of the atonied muscles, and eventually improve the tone of the entire system; thus, if sufficient time is allowed, the patient may be restored to health and to the active enjoyment of life.

After intimating his intention of continuing his observations on the same subject the succeeding Monday, Mr. Thomson exhibited to the class a child, five years of age, having the antero-posterior curvature. This had existed about a year, and implicated the five lower dorsal, and the three upper lumbar vertebræ. The case was taken into the hospital.

SOME OBSERVATIONS ON THE

STRUCTURE AND PHYSIOLOGY

OF THE

EYE AND ITS APPENDAGES.

BY DR. KNOX,

Corresponding Member of the French Academy of Medicine.

The anatomy of the eyeball attracted my attention almost from the earliest period of my anatomical studies, so that nearly thirty years have elapsed since I first examined the structure of the human eye. This circumstance, perhaps, has led me to give to everything said or written about it all my attention; nor as a dissector of the instrument itself throughout almost all classes of the animal kingdom, have I been, so far as I am aware, either careless or impatient. My present intention is, should leisure permit me, to review some of my own opinions and statements, contrasting them with those of my contemporaries, or of my more immediate predecessors.

Since the period when I first could reason at all about the action of the muscles of the orbit, I became fully convinced that of the six muscles whose tendons are inserted into the sclerotic tunic of the eyeball, two at least must act as the antagonists of the other

four; that the four recti muscles are, by their osseous connections and their position, the muscles whose combined action must be to retract the eyeball within the orbit, not merely occasionally, nor always according to our will (which, however, they very generally obey), but by a constantly acting muscular force, common to them with all the muscles of the body; and that this unceasing muscular action would finally terminate in the destruction of the eyeball itself, were it not for the antagonising power of the oblique muscles, whose function has ever seemed to me to be simply the drawing the eyeball forwards as the recti draw it backwards; that by these antagonising forces the eyeball is kept in its position, nicely balanced, easily turned upon its axis, and, in short, much in the same physiological condition as any segment of the limbs, a finger, for example, or the hand, or the forearm, whose motions obey the slightest impulse of our will, simply, as I imagine, from the nice balance of the antagonising muscular forces acting upon them. I am quite sensible that to the more refined and wordy physiological school of the present day, all this may appear too simple, and to others it may seem anything but new. This latter point I will not dispute; there is not much new either in anatomy or physiology at any time. De Graaff, Morgagni, Santorini, the Hunters, looked into anatomical structure with as nice and various an eye as any modern, and were fully as competent to decide on the physiology of animal organs as we of the present day. But this I will maintain, that whilst, during the last fifteen years, I have publicly supported these opinions respecting the uses of the muscles of the eyeball, and have further stated it to be my firm conviction, and this annually, in the presence of large classes, that no cruel vivisections, no experiments, as they are called, upon the lower animals, could possibly be required to confirm statements demonstrable on the dead body, or deducible from laws regulating muscular action; still there have not been wanting many who have thought quite otherwise, and who, either doubting the correctness of these opinions, or imagining that however true, they did not contain the whole truth; that is, have believed that certain of these muscles perform other functions besides those I have stated, have proceeded from reasoning to experiment, attempting to solve by the knife, or by vivisection, doubts which, according to my humble opinion, ought never to have existed. Perhaps the simplest way to state this question will be to determine, first, what may be demonstrated by the dissection of these muscles in the human eye after death.

The muscles of the adult eyeball having been exposed and dissected, they were pulled in a direction corresponding to their axis, and so as to imitate their contraction during life:

1st. The superior oblique turned the pupil downwards and inwards, and drew the eyeball forwards.*

2nd. The obliquus inferior, when acted on *alone*, directed the pupil outwards and upwards, bringing the eyeball itself *forwards*.

3rd. When both oblique were acted on or shortened together, they pulled the eyeball *forwards*, and directed the pupil downwards and inwards.

4th. When the recti, as well as the oblique, were acted on at the same moment, so as to imitate their combined action during life, then the eyeball became fixed, and the pupil looked straight forwards.

That this must be the action of these muscles no one, I think, can reasonably doubt; but some ingenious men have thought that the recti, by compressing the eyeball, may perform some other function, and more especially that of altering the focus of the eye so as to adapt it for vision at various distances. Looking attentively at the length of the recti, the distance between the posterior surface of the eyeball and their osseous attachments, the cushion of fat surrounding the eye towards its back part, I hold this opinion to be opposed to all the anatomical facts. If we direct a person to look attentively at an object placed within a moderate range of vision, say two feet, and then cause that object gradually to approach the eye till within a few inches, vision will continue distinct, until the effort becoming too painful to the individual, the object is all at once lost sight of, and becomes, as it were, invisible, until the eye has recovered from the effort that has been made. In the meantime, during this gradual approximation of the object to the eye, say from two feet to within a few inches, the only perceptible change in the eyeball itself is a progressive contraction of the aperture of the pupil, which seems, at last, to reach a point beyond which this contraction can be no longer endured. Vision is then lost for a moment, the pupil dilates to regain afterwards a medium state of contraction. If we now, as it were, reverse this experiment, and holding up an object before the eye of a person, at a distance of about two feet, and having requested him to look for a short time attentively at the object so placed, and then suddenly to extend his vision to a distant hill, there takes place in the pupil so manifest a dilatation as almost to induce us to believe that the motions of the iris are to a great extent under the command of the will. It may be said, however, that these experiments are not conclusive against the supposed action of the recti muscles, and I shall, therefore, mention another fact which bears somewhat

* If acted on more strongly it turned the eye outwards.

on this point. If the recti, by their action, and more especially by pressing upon the eyeball, enable us to adapt the eye to various distances, how comes it that we should feel almost equal fatigue in regarding very remote and very near objects, straining, as we say, the eyes, and necessitating their repose for a time? The same muscles cannot do both. They cannot both lengthen the eye and shorten it; they cannot suit the eye as well for near and for distant vision; and it is worthy of remark that when we press the side of the eyeball pretty firmly with the finger, thereby altering its form, and flattening and lengthening it, we may convert the eye, as it were, into a magnifying glass of very considerable powers, and an object held up close to it may be seen not only much nearer then than at other times, but also appears magnified to double or even more its usual size. It must, I think, be owing to the unusual spasmodic pressure exercised upon the eyes in violent passion, in ungovernable rage, for example, or by dread and superstitious fear, that objects appear magnified and close at hand. Hence persons in this condition are represented not only as confused in their own movements, but as magnifying the danger, and striking short of the object of their fear. Double vision, under such circumstances, is more easily explained. The person has lost the power of applying both eyes to one object; hence he sees double; and hence it was no trifling beauty in the poet to represent Orestes, whilst under the influence of terror, as seeing a double Thebes and two suns blazing in the firmament.

It is obviously, then, very difficult to dispose of the question as to whether or not the recti, in contracting, besides having a tendency to withdraw the eyeball within the orbit, do at the same time, by pressing on its structure, give rise to such an alteration in the form of the eyeball as to modify its shape, altering thus the focus of the retina, and even slightly pressing the eyeball outwards; but in this I do not myself believe. As to the opinion of those who think that the recti muscles do not act excepting under the influence of the will; that they are strictly voluntary; that they cease acting when we sleep, &c.; all such opinions are absolutely in contradiction to the well-ascertained facts respecting muscular action.

I have, all my life, had a natural horror for experiments made on living animals, nor has more matured reason altered my feelings with regard to these vivisections, but rather confirmed me in the belief that for the most part they are wholly unnecessary, and, therefore, highly to be reprobated. A minute and careful anatomy, aided by observation of the numerous experiments made by nature and accident on man himself, seem to me to present infinitely the best and surest basis for physiological and pathological science. It was not, therefore, without regret that I perused an account of some experiments on living animals made by Mr. B. Cooper, to determine the action of the muscles of the eyeball, more especially when these experiments lead to results ascertained many years ago by the anatomy of the dead textures.

The experiments alluded to are detailed in the Seventh Number of "Guy's Hospital Reports," and by means of them Mr. Cooper has come to the following conclusion:—"Five experiments were performed, in each of which the superior oblique muscle was divided in the one eye, and the inferior oblique in the other; that one in which the superior oblique had been divided never presented the same degree of prominence as the eye in which the inferior oblique had been cut through, seeming as if it were preternaturally withdrawn into the orbit, while the eye in which the inferior oblique had been divided looked as if projected, which, however, I think, is only attributable to the contrast between the two." Now, that this conjecture is the true one (although Mr. Cooper has taken up a contrary idea in a future part of his memoir), was afterwards proved by the following experiments:—"*In one or two of the rabbits I divided both the oblique muscles in the same eye, the result of which experiment was, the permanent retraction of the globe within the orbit.*"*

That these experiments, then, simply prove (and no more) an opinion which could be deduced, and had been deduced by myself, and by others no doubt, from the anatomy of the muscles of the orbit, and from the usual physiological laws of muscular action, will scarcely, I think, be now doubted by any candid person; they determine at least one, and that, unquestionably, the principal, function of the oblique and recti muscles of the eye, viz., that they are the direct antagonists of each other; that both oblique draw the eye forward; that the recti retract it, and, by antagonising each other, they enable the will to direct the axis of the eyeball with the most perfect ease and freedom in every possible direction.

But, although these results be manifest, other questions, undoubtedly, may be started in respect to the action of these muscles; do they all, for example, equally obey the will? or are the recti more especially voluntary muscles, the oblique involuntary? How are they influenced by the passions? And, finally, do they individually or combined contribute to other

* It is sufficiently curious that some years before, Sir Charles Bell, experimenting on the same muscles in the live monkey, had arrived at an exactly opposite conclusion.

functions than the merely mechanical ones of antagonising each other, giving to the animal the power to move the globe of the eye in all directions, and of maintaining, by a nicely-balanced pressure, the tension of the ball necessary to distinct vision. These questions cannot be altogether satisfactorily solved. That in passion the eyes seem ready to start from their sockets, is a common observation. That this is caused by an undue action of the oblique muscles seems to me equally manifest. That the violent action of all the muscles during passion alters the form of the eyeball, magnifies objects, and renders vision double and obscure, requires neither experiment nor reasoning to demonstrate its truth, the facts being matter of common observation. But, why are the eyes occasionally turned up on the approach of sleep, not merely in man, but in some of the lower animals, the dog, for example? And why do they turn up in hysterical women, and in children with excitable nervous systems? And, finally, why do they turn up, as has been said, on the approach of death? These questions, we shall find, have never yet been fairly met. I must, first, take leave to enter my formal protest against the assertion of some distinguished physiologists that the obliqui are involuntary muscles, and that being so, the ball of the eye on the approach of sleep, (the recti, as *voluntary* muscles, at that moment losing their influence), is, as it were, handed over to the exclusive action of the oblique muscles, which then preponderating direct the pupil of the eye upwards, and inwards, beneath the upper eyelid, and that the same phenomenon takes place on the approach of death. I must here positively declare that after many hundred observations made particularly in hospitals (where such opportunities of observation are, unhappily, but too frequent), I have never seen, but in a few nervous and hysterical women, and in a very few children, the eyes turned upwards on the approach of sleep or death; and in the many hundreds of brave men I have seen die in foreign and domestic hospitals, I never once observed such a phenomenon. Instead, then, of this appearance being constant, or even of frequent occurrence, it is, on the contrary, exceedingly rare. In respect to the lower animals, I have never observed the eyes to turn up on the approach of sleep, excepting in the dog; and unless from wounds of the brain or spinal chord, the eyes never turn up as death approaches, but remain generally fixed, with the pupil directed forwards.*

Neither physiology, then, nor anatomy, as yet offer any rational explanation of the causes why certain passions give a peculiar direction to the eyes, any more than to the hands or trunk; nor can any reason calculated to satisfy a thinking person, be assigned why so many nerves should proceed to the orbit, and some be distributed solely or chiefly to particular muscles. As to the theory of an excellent mechanical philosopher (Mr. John Russell), that to the oblique muscles of the eyeball must be assigned the function of the adaptation of the eyes to varying distances;—to this theory I need not reply; it is based on a mode of reasoning which, however sound it may be in general physics, is altogether inadmissible in physiology. Mr. Russell's *argument* (for it merits no other name) is, that as no *other use* can reasonably be assigned to the oblique muscles, their action must necessarily be that assigned by him, viz., the adaptation of the eyes to varying distances. But I was not aware that such a mode of reasoning had been admitted in natural philosophy or in mechanics; it seems rather to belong to that wordy division of human knowledge (?), logic. The *argument* would look better and fresher in the hands of a college wrangler, with whom words and sophistry stand in the place of facts and common sense, and to whom the deduction from the premises is everything, the premises themselves nothing.

(*To be continued.*)

* The eyes of birds frequently turn upwards when they die from an injury done the brain, but death from other causes does not give rise to this appearance.

CEREBRAL AND SPINAL PARALYSIS COMBINED.

To the Editor of THE LANCET.

SIR:—Hoping that the attention which is now directed by the scientific members of the profession to the doctrines of muscular irritability in paralysis, will form sufficient excuse for troubling you with the annexed case, which has been, and still continues, under my care, I remain, yours obediently,

C. H. ROGERS HARRISON.

18, Southampton-street, Fitzroy-square.

R. B. P., Esq., of gouty diathesis, and apoplectic tendency, was suddenly seized, on the 21st of February last, with giddiness in the head, and failing to reach the wall for support, fell heavily in the street, and struck his head against the flag-stones on the right antero-lateral portion. He was taken up insensible and conveyed home. When I saw him, within an hour (5, P.M.), his symptoms were, complete paralysis of the left side and partial of the right lower extremity; the right pupil contracted to the light, the left one remained immoveable, and dilated; the left angle of the mouth was drawn down; the hearing on the left side gone, and the sterno-cleido-mastoideus and scaleni muscles of that side violently con-

tracted; breathing heavy and laborious, with slight stertor; pulse full, and struggling, and 104, although he had been cupped to sixteen ounces a few minutes before I saw him. Venesection of the arm, to sixteen ounces, was employed, and a strong mixture of Epsom salts, senna, and jalap, given every two hours. 10 o'clock, P.M. Pulse quiet, and 80; medicine not operated; continue it.

22. 8, A.M. Remains the same; no effect from the medicine. Ordered one drop of croton oil in a pill, to be taken directly. No urine having passed a catheter is introduced, and about a pint of thick, high-coloured, strong-smelling, water is drawn off; still remains insensible. 8, P.M. No improvement; and the medicine not having had the wished-for effect, an enema is administered, containing Epsom salts and jalap, which, after being contained about an hour, produced six or seven consecutive involuntary evacuations; the urine also passed involuntarily; but the patient has so far recovered his senses as to ask for his hat and spectacles, which had come off in his fall.

23. 8, A.M. His remembrance quite restored, and talks of the events which immediately preceded his attack; complains of great pain in the left arm and foot, but has not the slightest power of motion. Quietness only enjoined. 8, P.M. Complained of distracting pains in the head; pulse 110; is very restless. Venesection to 16 ounces. Ordered,—

Calomel, 5 grains;
Acetate of morphia, a quarter of a grain.
A pill, every sixth hour.

24. Left pupil sensible to light, but vision much impaired; pains in the left arm and leg tormenting.—Omit last evening's medicine, and give, instead, one drop of croton oil.

25. Passed a restless night; pain in the head severe; pulse 70, and sharp.—Repeat the blue pill and morphine every four hours. Fæces and urine pass involuntarily.

26. Pulse accelerated; attributed to the effect of the mercury, which is omitted; pain in the head continues; quotes long passages from the plays of Euripides, the Iliad of Homer, and Inferno of Dante. Excoriation of the skin on the left trochanter, left external tuberosity of the femur, and outer ankle of the same side.—Omit all medicines, and recommend quiet.

27. Pulse nearly natural; head much relieved; use of the right leg quite restored; sloughy appearance of the skin covering the left trochanter. Apply a little spermaceti and a soft dry pad. Still displays surprising powers of memory, and speaks of himself with great interest. Fæces and urine pass involuntarily. No medicine.

March 5. Has remained in much the same state as last report, only that the sores have healed kindly. Broth diet.

Strychnine, one-sixth of a grain;
Calomel, 1 grain.—One pill thrice a day; and tartar emetic ointment to be rubbed along the spine until pustules appear.

9. No visible effects from the medicines ordered above, which he has continued to take, so discontinue them; pulse 80; no pain in the head. Diet the same as before.

11. Pulse 88, and rather full; the bowels not having been opened for two days, ordered,—

Extract of aloes, 5 grains;
Powder of scammony, 5 grains; with enough of Venice turpentine to make two pills.

12. Noon. Pain in the head returned; pulse 120. Venesection to twelve ounces; repeat the pills; omit the broth. 10, P.M. Much relieved from the morning's treatment, yet complains of pain in the paralytic arm and leg; vision not much improved.

13. 9, A.M. Passed the easiest night since first ill; contraction of the neck on the left side given way; has an idea he has caught cold, and has rheumatism in the left arm and leg, on account of the pains, which are severe, in the ball of the great toe. Ordered beef tea.

20. Continues improving; has taken an occasional pill, as prescribed above; pain in the head entirely gone; hearing on the left side slight.

27. Still improving in health; but has now well-marked gout in the left hand, and feels the pain severely; has not the least power of moving, as, when requested to do so, his endeavours terminate in moving his right hand and muscles of his face impatiently.—Ordered 15 grains of soda three times a day.

28. Finger-joints of the left hand inflamed and swollen, and painfully tender to the touch, whereas the arm is not susceptible to the impressions of pinching and biting.—Continue the soda, with the addition of six grains of rhubarb to each. Fish diet. Pulse 75, and rather small.

April 8. Greatly improved in general health; pulse 70; gout still continues, although inflammation and swelling have nearly subsided. Yesterday (7th), for the first time, twitched up his left leg on the excitation of tickling the sole of the foot; angle of the mouth brought to its straight line.

10. Bowels being confined, ordered two of the aperient pills prescribed on the 12th March.

11. Urine passes freely, but bowels remain confined; pulse 80.—Repeat the pills, to be followed by a draught of salts and senna, if not effective in six hours. Pain in the head makes him restless.—Omit fish and beef-tea, and take warm gruel.

12. Mid-day. Bowels not acted upon.—

Ordered one grain of croton oil, to be repeated in four hours, if not previously relieved. 8, P.M. I was sent for, in great haste, to see the gentleman, who, I was told, was dying, and when I saw him he was in the most profuse sweat I ever witnessed; it ran from his head and face in streams. Inspiration was long and heavy, expiration short and sharp; pulse barely perceptible, and very rapid, at least 140; perfect sensibility of the sensorium, and felt quite resigned to his fate, which not he alone thought was nearly closed. A teaspoonful of brandy was instantly administered, and repeated at intervals until his pulse increased in volume and decreased in number. A powerful enema was then given, as well to clear the bowels as a precaution against too great reaction in the circulatory system.

13. Passed a tolerable night; no return of the palpitation; bowels continue acted upon, but passed *with* knowledge; pulse steady and good; slight voluntary movement is perceptible in the left leg; complains of the feeling of pricking in the paralysed hand and arm; gouty affection of the finger-joints going off.

14. Improving; pulse 60, and small. Half meat diet.

15. Had a slight remission of the palpitation. Ordered friction over the precordial region with tartar emetic ointment, and an aperient draught to be taken occasionally.

25. Has continued improving in general health.

28. Convalescent; his present condition is, vision greatly improved, as also the hearing; tongue clean; has very slight command over the left arm, more over the left leg; excretions natural, with the occasional help of medicine; no pain in the head; pulse 70, and easily excited.—Ordered quiet, and generous diet.

OBSERVATIONS.

The above case appears to offer one or two points of practical importance in respect of nervous influence on muscular action and muscular irritability.

If we look minutely into the case we shall find that here we have a combination of what Dr. Marshall Hall calls cerebral and spinal paralysis, inasmuch as the former accounts for the hemiplegia and diminished irritability; and the latter, paraplegia and increased irritability of the rectum and urinary bladder. Following up the record of the case, we find that the first thing complained of (when the sensorium had recovered its function) was excruciating pain in the hemiplegic hand, arm, and leg; pain not accompanied by muscular irritability, for although he had not the power of volition over the paralysed limb, yet sensation was acute. Now, as the motor nerves of muscles are not susceptible of intense pain (if any), we must infer that the nerves of sensation coming from the roots which were affected, and the nerves of volition neither increased nor diminished in irritability; and if this be true, then we have sensation preceding volition, and, consequently, must look elsewhere for the doctrine which gives volition the precedence of sensation.

As to the paraplegia, which, after the indefatigable investigator, Dr. M. Hall, I venture to call spinal paralysis, we find *no* pain, nor yet power of volition, consequently, the nerves of sensation not diminished or increased in influence, but the nerves of volition diminished at first but afterwards increased.

Again, the exhibition of strychnine threw no light upon the subject, as it had no visible effect upon *any* muscular fibre, whether paralysed or not, in this instance; although it may be said it was not given to a sufficient extent to produce irritation.

The kindly healing of the sores occasioned by wet and continued position of the limb, shows how the vital principle of regeneration was as active as in a healthy limb, and the subsequent exhibition of gout proves that paralytic limbs may become the seat of disease common to healthy parts, an instance of which I heard a short time ago, where a perfectly paralysed limb was broken, and united by bone, the patient experiencing no pain during the operation of new growth.

DISEASES OF THE SPINE.

To the Editor of THE LANCET.

SIR:—I was pleased to see in THE LANCET of the 20th inst. the "Substance of a Clinical Lecture on Curvature of the Spine, by Frederick Hale Thomson, Esq." It is a subject to which the attention of the profession may well be especially directed; anything, good or bad, which may be the means of exciting that attention, must be productive of benefit, and, therefore, it is that I venture to solicit, in your pages, a place for this communication.

The treatment of distorted vertebral column (as Mr. Thomson justly observes), has remained unaltered from the days of Galen until now. Quiescence, recumbency, and counter-irritants formed the staple of Galen's treatment, and such has been the treatment adopted by most of his successors down to this day, and inclusive of Mr. Thomson himself. It is certain, however, that very many, before Mr. Thomson, have insisted earnestly upon attention to the general health; and there is no name more justly celebrated in that respect than that of my late instructor and friend Mr. Abernethy. I think the lecturer in error, however, when he states that "the first step of the treatment

should be to improve the general health." The first step of all is, assuredly, the choice of position; without IT no other treatment can avail; and, although it may seem trifling thus to dispute about precedence where we agree upon principles, I feel impelled to do so from the knowledge that thousands have been, and that hundreds are now being, sacrificed, who might, if *position* had been primarily and strenuously insisted on, have been rescued from disease, distortion, and death.

Amongst the many distorted objects who come under my charge, there are few who have not been directed by some physician or surgeon to attend to the general health, while small is the number of those who have been enjoined a strict adherence to the best *known* position; and thus, although the questions of diet and medicine may have been sometimes properly attended to, the disease and the curvature have necessarily proceeded until the form, and with it, perhaps, the health of the patient, have been irretrievably destroyed.

I knew a young gentleman, four years of age, and, indeed, for a very short time, had the direction of his case, who was ordered, by a surgeon of high eminence, to refrain from lying constantly in any position, and who was, therefore, allowed to sit and run about at will, although disease and consequent distortion were going on in two distinct portions of the spinal column. The consequence has been a great increase of the curvature without a corresponding improvement in health. On the other hand, I may mention the case of a little girl, about the same age, whom the immense discharge from two abscesses connected with diseased vertebræ, appeared, during many months, to be dragging to the grave, who, after lying unmoved for nearly two years, has now recovered her health and strength, while the distortion has not only been prevented from increasing but has even very considerably diminished. For such reasons it is that I hold the choice of position to be the paramount consideration, and, that point settled, the remedies, whether constitutional or local, may be applied with great chance of success; but on the question of position there is a difference of opinion between Mr. Thomson and myself; his directions are that the patient be kept flat on the back, and lie on a hair mattrass; that, however, I venture to affirm, is far from being the best position. Some of my reasons for saying this I have given elsewhere, and I shall hope one day to descant more largely on the subject. Space could not be allowed me here to say what I could wish in support of an opposite mode of treatment; I must, therefore, content myself with affirming that I believe there is no surgeon who has carefully and candidly watched it who has not become a convert to its general superiority.

Mr. Thomson's statistics appear to me to be very erroneous; his statement that among the poorer classes these diseases are comparatively rare, is deduced, I think, from a very partial view of the case. The peculiar character of his practice brings him into contact chiefly with the more affluent classes, while the charitable society whose servant I have the honour to be, calls on me continually to visit the most wretched abodes of poverty, destitution, and disease; my calculations, therefore, are based upon data very dissimilar to his, and are productive of very different results. I should say that while lateral curvature occurs most frequently in the upper and middle ranks of society, posterior curvature, the effect of accident or disease, is more than proportionably predominant amongst the poor.

Thus, while Mr. Thomson, out of his 300 patients, enumerates only 37 cases of this description, I have at the present moment 76 cases of diseased spine, with posterior curvature, under my immediate care. A very large proportion of these are amongst the very poor, and the greater number of them have fallen victims to the inert, undecided, and injudicious treatment of which I find so much reason to complain. Some of these, however, have been subjected to other, and I may say better, treatment, founded on the use of the supine position, but in which pain, excoriation, and probably increased and increasing ill health have forbidden them longer to persevere. I dwell on these statistical questions that I may show how lamentable a thing it is that in this country, so famous for its numerous and noble charities, there has never, until now, been any society having for its object the treatment of this class of disease, and that there is not even yet any institution for the reception and relief of the deformed and destitute objects who throng the courts and alleys of this immense metropolis.

I have rarely read anything on the subject of diseased and distorted spine with which I have been thoroughly satisfied. There is too often a vagueness and confusion in the statements and descriptions which contrast unfavourably with the clearness and accuracy with which medical subjects are most usually treated. From these objections the abstract in question, perhaps not the original lecture, is far from being exempt. We have, for instance, in the third paragraph, the following passage:—"The affections of the spinal *chord* might be divided into two classes,—1. Those affecting the spinal marrow; and 2. Those implicating the bony structure and intervertebral substance alone." Affections of the spinal *chord* affecting the spinal marrow, has something very odd about it. For *chord* we assuredly should read *column*; but that is a trifling, perhaps a typographical error. But, for very evident reasons, reasons involving

the most important practical considerations, the division ought to have been tripartite.

1. Affections or diseases of the spinal marrow and its investing membranes.

2. Diseases of the column itself, including the bones, ligaments, and intervertebral substance, and inducing or accompanied with posterior curvature.

3. Lateral or twisted curvature, not accompanied by, or productive of, disease or disorganisation.

This is a division of the highest possible importance, and not one of those minute subdivisions which lead only to slight modifications of practice. The distinction here drawn demands a place in the vestibule, or rather it ought to appear on the very threshold; that it has not so appeared has been the cause of many errors, some of which it has been my lot to witness. I have seen, for instance, patients with lateral curvature tormented and even rendered worse by blisters, issues, setons, &c., which never could have done any good. Amongst the thousands of medical practitioners and students we must not expect that all are capable of thinking for themselves; they will have done well who have collected and have learned to apply the accumulated wisdom of ages as it may be found in books, or as it is delivered from the lips of the professor; for these it is necessary that those who profess to instruct, whether from the chair or through the press, should place such important distinctions in the strongest point of view.

Scarcely less important is another common error which also pervades the abstract, that of confusing together, or rather of making an indiscriminate use of the words distortion and disease. Thus, the lecturer is made to say that Galen's remedies for distorted vertebral column were quiescence, recumbency, and counter-irritation; and he himself is made to insist on "physiological principles," upon "constitutional treatment," &c., for the cure of distorted spine. Constitutional treatment and counter-irritants for posterior curvature! Why, not a single hunchback was ever cured by such means All the blue pills and grey powders, all the rhubarb, quinine, and sarsaparilla, together with all the iodine and camel's hair brushes in London, would be but thrown away upon it. They may be very useful as the means of curing DISEASE, but the word constantly used in the abstract is DISTORTION, and over this they can be of no avail.

Again, let me say that this is not a mere war of words; the author and the lecturer should be clear and explicit, or he will lead those to error who unthinkingly take him for their guide.

A surgeon of considerable practice and experience was desirous, a few days ago, of applying issues to the back of a young gentleman under my care, although every symptom of *disease* of the vertebral column has for some months disappeared.

Again, we have to complain of the loose and indefinite way in which the lecturer is made to speak of the disease or distortion, whatever it may be, which he professes so certainly to cure. For what is the patient to be laid on his back, and to have the strips of tincture of iodine applied? What was the state of the little boy in the Borough, who, at three years old, had been cruelly subjected to "every variety of the mechanical treatment?" What, in short, is the nature, and what the extent, of the evils which are thus to be perfectly cured in the short space of six months? Is it simple inflammation of the bones, &c.? Is it posterior curvature with or without anchylosis? Is it caries? Is it abscess? Is it paralysis of the lower extremities? These matters ought to have been explicitly pointed out in the lecture, and to have been especially recorded in the abstract. The descriptions of the lecturer are heard only by a few, but the abstract in THE LANCET will be read by thousands. It seems to me impossible that Mr. Thomson could have held out in any of the cases supposed the certainty of a cure in six months. He knows too well how many cases of such disease there are which will not yield in six months, whatever they may do in six years. Connected, as he allows them often to be, with scrofulous diathesis, there must, of course, be many cases that resist every attempt to cure.

I was called last autumn to the granddaughter of one of the highest dignitaries of the Church, who had had the first advice that London or the country could afford, who had lain flat on her back for nearly three years, until she was sinking into the grave, and who only changed her position in the hope (and she was not disappointed) that she might be saved during the last few weeks of her life from the terrible sufferings to which she had been long condemned. That incipient disease of the column in which there is not yet destruction of the parts, may be cured in six months, and even in a shorter time, I have many cases to prove; but the great body of the profession who, accustomed as they have been, to attack the enemy with the same weapons as Mr. Thomson, have frequently been baffled, and ultimately failed in their attempts, will agree with me in complaining of the tone and character of the abstract, which is positive only where some uncertainty must necessarily prevail, while it is confused and incorrect where clearness and exactitude were especially required.

Having thus pointed out my objections to the abstract, it is right that I should state that I agree with Mr. Thomson in preferring the milder counter-irritants, or rubefacients, either to the tortures of moxa, or the wasting discharge of setons and issues; and I

also agree with him in considering collars and stays, and backwards and spinal supports to be worse than useless, where the only safe mode of treatment must be based on a position affording perfect rest. I am, Sir, &c. &c.

CHAS. VERRAL.

Lowther Arcade, April 30, 1839.

INQUESTS IN MIDDLESEX.

POST-MORTEM EXAMINATION OF A MALE INFANT

Found Dead in Clarendon-place, Hyde Park.

THE body was discovered on Saturday morning, April 20th, at about eight o'clock, carefully packed up in a deal box, apparently made for the purpose, with a lid of the same material, and the whole was covered with brown paper, by which the action of the external air was pretty effectually excluded. It was in a flexed position, upon the right side, and, without removing it from the box, presented a portion of the left side of the neck, denuded of its cuticle, with a florid red, and moist surface, around which there was a margin of bluish green and white mould, the same kind of mould existing in patches over the outer surface of the left buttock and thigh, the front and uppermost part of the thorax, and the whole of the left cheek. The portion of the umbilical chord remaining attached to the abdomen was now observed to be unusually long, considerably shrunk, not at all desiccated, and of a violet colour, as from incipient putrefaction. No further remarks were made at this time, but the box, with its contents, was given into the safe custody of the police, to await the coroner's inquest.

On Wednesday morning, April 24th, the following appearances were noted:—

Weight of the body seven pounds nine ounces avoirdupois; length twenty-one inches and a half. Of the head, the occipito-frontal diameter measured four inches and a half; from one parietal bone to the other three inches and a half; the circumference taken over the occipital tuberosity and the frontal sinuses thirteen inches and a half; the anterior fontanelle measured about one inch from each parietal bone to its opposite frontal bone; the posterior fontanelle was nearly closed; the cranium was altogether as firm as usual at the natural period of birth. The thorax, to ocular inspection, and compared with the size of the child, was small, and not very well formed; it was sufficiently wide at its base, with the sternum elevated, but narrow at its apex. The shoulders measured four inches and three-quarters across. There were no marks whatever about the child indicating difficult or preternatural parturition. The general surface, with the exception of its denuded and moulded portions, presented a fresh appearance, with sugillations on the back and dependant parts. The entire scalp was of a remarkably vivid red colour, and plentifully covered with long dark hair; in this situation, and under the mould on the surface of the body, the cuticle was very easily detached, and on removing it the moist cutis was of a florid red colour. This was not the case in other parts; the moulded patches were surrounded by a dark brown, well-defined areola, about three lines broad, and this, again, by a light purple shade, about twice the width. The mould, examined with a powerful microscope, exhibited no signs of mineral crystallisation, but was of a vesicular structure; the rest of the surface was perfectly free from the unctuous coating peculiar to new-born infants. The eyelids were closed, prominent, somewhat puffy, and their inner surface of an uniform dark-red colour; the tunica adnata had several dark-red spots, as if ecchymosed; decomposition had so far advanced that the pupil could not be discerned. The remaining umbilical chord was seven inches and three-quarters long, twisted, and tied on itself into a single knot, about two inches and a half from the abdomen; its extremities were either torn or cut very irregularly. The centre of the body was three-quarters of an inch above the umbilicus. The testicles were in the scrotum. The nails protruded beyond the fingers and toes. In all other respects the external formation was perfect and well-proportioned.

Immediately on opening the thorax the lungs were observed to be of a pale bright-scarlet colour over the whole extent of their surface; they covered the sides and arch of the diaphragm, leaving only a small strip of its anterior border exposed. The lower margins of the left upper and right middle lobes were neither sharp nor perfectly rounded; there were two or three vesicles upon their surface, about the size of half a kidney-bean, split longitudinally, and, here and there, between the lobules, the membrane was distended with air; they were crepitant throughout their substance. On incision they presented the same bright-scarlet colour, and by slight pressure blood oozed freely from their vessels, and froth from innumerable nearly invisible orifices; being isolated as completely as possible from the surrounding parts, they weighed 900 grains, or in the proportion of 1 to 64½, compared with the weight of the body. When thrown into common water, before the heart was detached, they floated close under its surface; after the removal of the heart they floated with a small portion above the surface of the water; when cut

into very small pieces, each piece floated, and no degree of compression could make them sink. The diaphragm was moderately arched.

The foramen ovale was completely open. The ductus arteriosus was collapsed and empty, five-eighths of an inch long, its diameter less than half that of the pulmonary artery, and somewhat smaller than that of either of its lateral branches; it was a little contracted at a point one-third distant from the pulmonary artery. The ductus venosus, umbilical vein, and umbilical arteries, were collapsed throughout their extent, but still pervious. The veins in the body generally were filled with fluid blood.

The *lips*, and the mucous membrane lining the interior of the mouth, were of an extremely dark-red colour, the latter being covered with what appeared to be a thick white secretion; the tongue was protruded against the inner surface of the lips, and partially between them; its base, as also the soft palate and upper portion of the œsophagus and larynx, were extremely vascular; the epiglottis was œdematous; the lower portion of the œsophagus was healthy, with its epithelium entire; the stomach was collapsed, extremely soft or gelatinous, and contained only a very small quantity of a thin, reddish fluid. On chemical examination this fluid gave no reason to suspect the existence of any corrosive or poisonous substance. The duodenum was in nearly the same condition as the stomach; the gall-bladder was collapsed and nearly empty, its internal surface being only thinly coated with a thick dark-brown fluid, not at all resembling bile; the small intestines were much less softened than the stomach and duodenum, they were throughout contracted and wrinkled, and contained only a very small quantity of a mass resembling meconium in consistence, but of a dull, pale, straw colour; the large intestines, from the cœcum to the commencement of the sigmoid closure of the colon, were completely filled with a similar pale, dull, straw-coloured viscous mass. At the spot last mentioned this mass abruptly assumed the usual dark-green colour of meconium; it was rather more fluid, and filled to distention the remaining portion of the large intestines to the anus; at the external orifice of the anus there was a small quantity of meconium. The bladder was empty and contracted.

On raising the calvarium, the brain was found extremely soft, the sinuses contained fluid blood; the membranes of the left hemisphere were very vascular; there was slight extravasation of blood on its surface, and a small quantity of blood was effused into the bony fossa which supports the middle lobe; the membranes of the right hemisphere were less vascular; the cerebellum, medulla oblongata, and medulla spinalis, exhibited no unusual appearances.

The more important inferences to be drawn from the above description are the following:—

1. That the period from the birth of the child to the finding of the body may have been several weeks.
2. That the child had obtained the full period of its fœtal existence.
3. That the child was born alive.
4. That the child was perfectly formed, and that although its chest and lungs were somewhat smaller in proportion to its bulk and weight than the usual average, it was, nevertheless, healthy and vigorous.
5. That no educated midwife was present at its birth.
6. That it lived but a very short time.
7. That it did not die from hæmorrhage, or from the effects of cold.
8. That the appearances are quite compatible with death from suffocation.
9. That some of the appearances on the exterior of the body and about the lips and mouth, lead to a supposition that it had been scalded with water, or some other fluid, during life, but that it is by no means certain that they were not the result of putrefaction.

HENRY ANCELL.

39, Albion-street, Hyde-park,
April 26, 1839.

HYDROPHOBIA TWENTY-SEVEN MONTHS AFTER THE BITE.

ENORMOUS DOSES OF

CROTON OIL AND PRUSSIC ACID.

SIR:—As hydrophobia is, happily, a disease of anything but frequent occurrence, and, as from the mysteriousness of its character, its treatment has been, almost necessarily, conducted on principles empirical rather than philosophical, I cannot help thinking that it becomes the duty of every practitioner who meets with a case, to give his professional brethren the results of the treatment, whether successful or otherwise; and especially if that treatment possess anything of novelty; and still more so, if it appear to have imparted any new or peculiar features to the phenomena which ordinarily characterise this awful malady. If, therefore, you deem the following case deserving a place in your pages its insertion will oblige yours very truly,

FREDERICK CRIPPS.

113, Dale-street, Liverpool,
April, 1839.

Mrs. H. (a respectable innkeeper's wife) was bitten on the face in the November of 1836, by a little terrier dog, which, from a variety of circumstances, there is every reason to believe was mad. The wound was

washed with salt and water; after some little difficulty it healed up and nothing more was thought about it.

On Monday, March 11th, 1839 (consequently *two and a half years* after the infliction tion of the bite), I had occasion to visit a grandson of Mrs. H. On asking Mrs. H. how she was, she said, "Very well, but my throat feels queer, though not sore." On the Wednesday this uneasy sensation had increased; she then told me she felt hot and feverish, but "thought it was only a cold and would, therefore, take nothing for it, as she should be well in a day or two." On Thursday evening her husband sent for me to see her; she was then in a state of high nervous excitement, not unlike that which presents itself in the earlier stages of delirium tremens; the face flushed; eyes suffused; skin dry, but not much above its natural temperature; pulse 102. She had had no sleep for two previous nights. The countenance was expressive of the deepest anxiety; she made great efforts to compose herself and be quiet, but said she felt as though she "must move about and talk;" indeed, the disposition to loquaciousness which has been mentioned by writers as incidental to hydrophobia, was well marked in this case. She was constantly using a cloth to remove a thick viscid mucus which collected, in large quantities, in her mouth, and very much plagued her. She expressed great fear lest the air in the room should be disturbed by the rustling of the curtains, opening the door, &c.; she dreaded the sight of water, nor could she even bear to hear its name. On asking her if she would try to swallow a little she shuddered; a small quantity, in a glass, was brought near to her, when she became convulsed; on pouring it into a basin she kicked, fell back in the bed, and the true hydrophobic spasm now manifested itself. On blowing into her face the same effect followed; indeed, she seemed to dread it, if possible, more than the water, and begged, "for God's sake," that that might not be repeated. Dr. O'Donnell, who had been sent for, had now arrived, previous to which I had administered a dose of camphor and opium.

The question now to be determined was,—What line of treatment should be adopted? Unfortunately, the past experience of others assisted us but little in determining this point; to combat symptoms, then, was the only alternative.

Now, had such a case never before been witnessed? Could such a series of symptoms have been attributed to a specific cause? In fact, if the existence of such a disease as hydrophobia had never before been known, there could have been no hesitation in declaring the phenomena before us to be dependent upon an inflammatory or highly irritable state of the nervous system, to allay which, would, of course, be the obvious indication. Now, to effect this, bleeding would, probably, first suggest itself; but if any reasoning could have led to the conclusion that the abstraction of blood would be of no avail (and such is the position which all practical precedent establishes), we should naturally seek, first, for some other depressing agent which might have the effect of allaying the inordinate excitement, and then endeavour to maintain that effect by some powerful sedative. With these views it was determined to employ two therapeutical agents, the activity of which, in their respective departments, is unquestionable, viz., croton oil and prussic acid. It will be seen that the doses of each that were taken, were truly enormous. With respect to their purity, I may observe, that the oil was "Short's," and the acid, Scheele's strength, made by myself from Laming's formulæ; and, in order to insure accuracy in the exhibition of the medicine, I, or one of my assistants, remained with her night and day.

Thursday, 7 P.M. One drop of the oil was given and continued every hour, until 15 drops had been taken. These caused about 17 or 18 motions, each successive one becoming less fæcal in its appearance, until at length the evacuations assumed a character precisely similar to the rice-water dejections of cholera; in fact, in almost all respects she resembled a person in the collapsed stage of that disease. There were the sunken features; the leaden hue; the clammy cold; the pulseless wrist; and, as before observed, the characteristic stools. She complained of great thirst, still there remained the abhorrence of anything which associated the idea of fluid; the spasms, however, were by no means so violent or so frequent.

Friday, 9 A.M. Dr. Brandreth accompanied us to see the case. It being thought that the oil had been carried to a sufficient extent, we proceeded to give the acid; two drops were accordingly administered in a little moist sugar; the dose was increased hourly.

2 P.M. The pulse became perceptible at the wrist, and ranged, during the exhibition of the medicine, from 96 to 120; the surface of the body felt warmer, and she expressed herself as being "much more comfortable." She said she "could think of water without shuddering, and thought she could drink a little;" but on trying to do so, a convulsion ensued, though much less violent than on the preceding evening.

6 P.M. No spasm since 3 P.M.; could now suck the corner of a cloth dipped in water.

12 P.M. Much the same, had had an hour's sleep; less thirst; pulse 102; no spasms.

Saturday, 6 A.M. No spasms during the night excepting a slight one produced by an attempt to swallow a little brandy and water, small portions of which she could take out of a teaspoon. The dose of the acid was now twelve drops per hour; this produced

no obvious effect, with the exception of the reduction of the pulse both in force and frequency, about six minutes after it had been taken.

12 A.M. The last dose that had been administered was 18 drops; I now gave 20, and the full effect of the poison developed itself. A few seconds after it had been swallowed, she gasped deeply, placed her hand near the precordia, and said "I am dying." Her eyes became fixed; the extremities rigid; a violent convulsive effort raised her head some distance from the bed, on which it again fell heavily; a quantity of frothy saliva escaped from her mouth; I could detect no pulsation in the carotid, and I really feared life was extinct. I immediately dashed some cold water in her face; this caused a convulsive movement, and she again began to respire; for about half an hour she remained in a state of the greatest prostration, but eventually regained her former state.

From this period the spasms almost entirely ceased; indeed, the only indication of their existence was what, under other circumstances, would be considered an hurried attempt at deglutition. She complained of no pain; her intellect appeared clear; bowels not relieved since last evening, and croton oil was again given, which soon produced an effect; pulse 96, and very feeble. The acid was again exhibited, but of course in diminished doses. Towards evening the debility was excessive, and appeared to be increased by each dose of the medicine; it was therefore suspended. She ate part of a biscuit and drank small quantities of weak brandy and water at intervals with the greatest facility.

During the night she had quiet dozes.

On the following morning (Monday) she appeared to be very calm; no return of the spasms; but though she drank freely, she still carried it up to her mouth with a slight jerk; quarter-grain doses of cyanuret of potass were substituted for the acid.

Towards evening she complained of a burning sensation at the pit of the stomach, but this ceased on diminishing the dose of the medicine. A copious perspiration broke out during the night; she slept soundly at intervals.

On the following morning (Tuesday) a fresh series of symptoms manifested themselves; the pulse became quick, skin hot, tongue dry and much furred.

I need not, however, further detail the case, inasmuch as the symptoms were now in every respect analogous to those of typhus fever;* and this identity was preserved until the following Friday, when, notwithstanding the greatest care and the most cautious employment of stimulants and other appropriate means, she died in a perfectly typhoid condition.

After the *complete cessation*, on the Tuesday, of all the symptoms of hydrophobia, and the substitution of the adynamic state, we had anxiously hoped to be enabled to place upon record, at least one successful termination of that awfully mysterious malady which, I fear, is still destined to baffle the skill of "Œsculapian science."

* We regret extremely that Mr. Cripps has omitted the details of the symptoms so analogous to those of typhus fever; and also to inform us of the post-mortem appearances in this interesting case.

THE LANCET.

London, Saturday, May 11, 1839.

We have already examined the more remarkable facts in the Army Medical Reports upon the West Indies, the United Kingdom, and British America; and have found the West Indies producing an annual loss of life which throws into the shade any commercial advantages that they may be supposed to yield, while the Canadas are as salubrious as the soil of England itself was in the same state of cultivation. The establishment of this fact is important, and encouraging to British emigrants, who are again likely to flock in thousands to the untenanted, unsubjugated, acres of America. It would appear from Lord Durham's able Report that the typhus fever which prevails in Lower Canada is introduced to a considerable extent by the emigrant ships from this country; as many as forty have died by contagious disease in a voyage of six weeks; and it is notorious that the number of poor emigrants who perish on the passage has, for a long time, been three or four times as great as the number of convicts who die on the long voyage to New South Wales. The emigrant ships are bound, by Act of Parliament, to have a surgeon on board; but the owners select their surgeons upon the principle hitherto upheld by the Poor-Law Commissioners, and the result has not been very dissimilar; disease has neither been prevented nor skilfully treated by the counterfeit Tender-surgeons.

Before proceeding to the Report upon the Mediterranean, and the discussion on malaria, we must set ourselves right upon one or two points. A correspondent complains, "that some injustice has been done to the surgeons of the Guards" in one of the preceding articles; but if the surgeons of the Guards consult the whole tenor of the article, in connection with the facts in the Report, they will come to an opposite conclusion. It was but justice to them to direct attention to the deficiencies in the Report of diseases both of the Foot Guards and the Household Cavalry, as they will find the following statement in the last Report, p. 9:—"The *medical returns* of this class of "troops [Foot-Guards] *not being all sufficiently minute in their details*, we are "unable to furnish the *usual particulars* "regarding the *number of admissions into* "*hospital*." The careless omissions of a few of their body deprived the observations of 1830—36 of their relative value; and necessarily laid the returns of those medical officers of the Guards who performed their duty,—that is, the majority,—open to a censure, which could not be too soon effaced. Our correspondent says that they are not better paid than their brethren of the Line; but that the returns are as accurately and fully drawn up as any furnished by other corps. We rejoice to find that this is now the case. The medical officers of the Army know that the results of their labours will be made public; and we assure them that while defects will be freely pointed out, and any faults censured in this Journal, none of them will ever be treated with injustice; as a proof of this, let them bear in mind that their merits, the value of the Army medical returns, the importance of their accumulated labours, have nowhere been so emphatically asserted as in THE LANCET.

This brings us to a criticism on the West India Report, by Sir ANDREW HALLIDAY, in the shape of a letter to Lord HOWICK. Sir ANDREW had the "good fortune" to be employed for a short time in the West Indies; he had "partially analysed the air from some "of the most pestilential marshes in British "Guiana," "had instituted a series of "experiments, and was pursuing them with "great ardour, when the *caprice of a silly*, "*passionate, old man, put an end to them at* "*an hour's notice*"!

Sir ANDREW, as might be expected after this, writes very much in the spirit of a philosopher interrupted in the middle of an experiment,—say the search after the philosopher's stone; there is much of pity, and more of anger, in his pages. The personal story merits little notice. But some remarks upon Sir JAMES MAC GRIGOR'S censorship, and upon the mental restraint imposed upon the medical officers of the Army, deserve attention. "Now, alas," says Sir ANDREW HALLIDAY, "all *writing men* have apparently "been excluded from the service. An "Army medical officer must neither take a "pen in his hand, nor open his mouth, "except within the walls of the military "hospital." The facts that the medical officers of the Army have latterly been silent on subjects of vital importance, that the few contributions which do appear, reach the press through the hands of Sir JAMES MAC GRIGOR,—and that interest and personal favour, rather than personal merit, seem to secure promotion,—lend a colour to this charge.

Sir ANDREW goes on to say:—

"It is to be most sincerely regretted that some officer in the medical department of the Army was not deemed worthy of being recommended to your Lordship, as capable of discharging those duties which were assigned to Captain Tulloch. It has tended much to lower the department, already sufficiently low in the estimation of the public, and must prove most disheartening to every member of that department, to find a military subaltern earning promotion by the performance of a task that most especially belonged to the medical officer. Army medical men have now but few opportunities of earning either praise or promotion, in or out of their department, and it shows but little zeal in their behalf, that they were passed over on this occasion. There are still many individuals connected with the Army Medical Department who are as able as Captain Tulloch, and not less willing, to undertake the examination and condensation of the Medical and War-Office Returns; and some,

I venture to assert, as conversant with statistical details, and as capable of turning them to account, as the gallant captain, or any other military officer whatsoever. I readily allow, notwithstanding, that no man could have more ably fulfilled the duty which your Lordship intrusted to him, than Captain Tulloch has done."

If this complaint were equally well founded, the medical officers of the Army would have a just ground of dissatisfaction; but Sir ANDREW has here, with a want of candour not at all creditable, suppressed part of the truth, for we find it expressly stated in the introduction to the "West India Report," that "Mr. HENRY MARSHALL, Deputy "Inspector General of Hospitals, and myself "(Capt. TULLOCH), were nominated to con-"duct the investigation by the Secretary at "War, on the recommendation of the Direc-"tor-General:" further on, Capt. TULLOCH says, that although Mr. MARSHALL left London in 1836, he "had the advantage of that "gentleman's revision, and, on all medical "points, of his assistance and advice." The Army is deeply indebted to HENRY MARSHALL; he almost led the way in Army Medical Statistics, and laid before the profession a series of positive facts which could not fail to promote medical science, and, in the end, to diminish the mortality of the troops, at home as well as abroad. Captain TULLOCH had also written several valuable statistical papers upon the mortality of officers and men in the "United Service Journal;" so that the Secretary at War, who could only judge of the capacity of individuals for the task by their *published* works, had no alternative; he was bound to do as a private individual would do, in the same circumstances, and to appoint men who could adduce the most distinct evidence of their qualifications.

Sir ANDREW HALLIDAY laments that the medical officers are never consulted "as to "the best construction of barrack-rooms, "or even of sick-wards." When he was "in the West Indies the medical officers," he says, "had entirely lost caste, and seemed "scarcely acknowledged." This must be partly ascribed to the supercilious ignorance of many English officers, who, as a body, although equal to any officers in Europe in valour and sense, are generally inferior to the French in scientific acquirements of every description, and naturally despise what they scarcely understand. But part of the blame attaches to the present system of medical education, to the neglect of positive facts, and of a philosophical method; and to the absurd, incoherent, unsubstantial traditions which one repeats and re-produces after another; and to a tone of unsupported dogmatism, which is little less than disgusting to healthy minds. Sir ANDREW HALLIDAY's pamphlet contains, amidst a few judicious remarks, innumerable illustrations of the current absurdities; take, for instance, his free and easy mode of accounting for the mortality, and the diseases of the troops stationed in the West Indies. He does not hesitate, without advancing a syllable of satisfactory evidence, to ascribe "the occasional explosions of dis-"ease which, in many instances, have "proved so fatal, *chiefly* to the *uncertainty* "*and inconsistency* which apparently have "too frequently prevailed in the changes "and removals of the troops in the com-"mand!" A ready mode this of accounting for epidemics. In another page he lectures the Secretary at War on freethinking, and declares that "*a due supply of the bread* "*of life*,—the Gospel of CHRIST,—will *more* "*effectually diminish the great loss of life in* "*the Colonies than any other measures that he* "*can possibly adopt*." Sir ANDREW adopts Dr. FERGUSON's dreams upon the origin of malaria; the passage is a good specimen of dogmatism in statement, and absurdity in doctrine:—

"In all these stations where the permeable nature of the earth allows the rain-water to penetrate rapidly deep beneath the surface, a wet season will prove unhealthy, because the moisture cannot all escape the malignant influences that give origin to the poison, and more or less of it, according to the quantity of rain that falls, will always be generated,—whereas the quantity that falls in a dry season, getting out of reach before it can be acted upon, the station will always be healthy. But, on the contrary, at a station where

the soil is shallow, and the rock or impermeable clay-beds are very near to the surface, the health of the troops will be the best when the season is most wet, for then the soil being so supersaturated with moisture, becomes as innocuous as the surface of the wide spread lake. There will be no fever while the rains continue, but sad will be the fate of those that are doomed to remain on the same spot when the dry weather has commenced."

The following is an hypothesis in which Dr. ADDISON *might have gloried* at the famous debate on malaria:—

"As regards the marsh-poison itself, and the question so often asked, What is it? I can only, like others, offer a conjecture, which is this: I consider that, at particular seasons, or under peculiar influences, certain living *essences* are called into existence, which, floating in the atmosphere, are swallowed in myriads, and dissolving in the stomach, poison the vital fluids; that, in proportion to the ravages committed upon these fluids we have the phenomena of fever more or less developed."

The *essences* are called *invisible but active insects*, in the next page.

Bowel complaints are not only frequent, but most fatal, among the troops situated in Barbadoes; and the worthy Knight is not astonished at this, for, "How can it be "otherwise," he asks, "when men, women, "and children are compelled to have re- "course to the same open and filthy privies, "where *vermin are generated in millions, and "often productive of most distressing and dan- "gerous bowel complaints?*"

The pamphlet ends with a profound aphorism:—

"No matter how high you pitch your tent; if under the range of 2500 feet, and if the hill is nearest to the plain or marsh (that is, the front in the range), you will never escape the ravages of the fever poison. And the higher you ascend the more virulent will it become. *If there is a still higher peak*, of the same range, adjoining your locality, even that higher peak will be unhealthy; *but the next beyond it, whether higher or lower, will be free from all danger.*"

Such "causes of sickness and mortality "can, indeed, never be discovered by any "accumulation of figures;" and, happily, neither Mr. MARSHALL nor Captain TULLOCH has attempted to grapple with the invisible insects,—to dive into the gravel of the mountain streams,—or to ascend the higher peak of mountains,—much less the next beyond it, whether higher or lower.

Sir ANDREW is a Knight by no means merry, though he might be the cause of mirth in others; and much as the reader may be disposed to smile, he cannot fail to be serious, when he reflects on the discredit which such extravagancies reflect upon medicine, particularly when it is too evident that these theories are mere exaggerations of doctrines that are every day taught in lectures and elementary books of physic. Dr. CORLAND's Dictionary, for example, erudite, valuable and instructive as it is in many respects, is filled with loose statements, a rash dogmatism, and a flagrant empiricism, which place medicine in singular and melancholy contrast with the other sciences, and almost raise a doubt whether the imperishable works of LOUIS could have been produced in the same age.

Chemistry of Organic Bodies.—Vegetables. By THOMAS THOMSON, M.D. &c. Baillière, 1838.

Introduction to the First Elements of Chemistry, for the Use of Students. By Dr. JUSTUS LIEBIG. Translated from the German by THOMAS RICHARDSON. Baillière, 1838. 2nd Edition.

Elements of Chemistry. By the late E. TURNER, M.D. Part III. *Organic Chemistry.* By Professor LIEBIG. Taylor and Walton, 1839.

THE first work on our list we have already noticed; but we refer to it again in taking a view of organic chemistry, as it constitutes the most complete system of vegetable chemistry extant. The second work forms the first part of "Geiger's Handbuch der Pharmacie," rewritten by Professor Liebig; for it is not correct to call it "Geiger's Pharmacy;" and the third book is an English version of the fourth part of the same publication. To complete, then, an English translation of the whole of the chemical portion of Geiger already published, we require only two parts which comprehend inorganic chemistry. We do not consider that we lessen, by this statement, the utility of Professor Liebig's part of Dr. Turner's

work; but we require to add that a mere literal translation would not have suited the English idiom. We are happy, therefore, in being able to state that the translation from the original MSS. of Professor Liebig has been exceedingly well executed by Dr. W. Turner, the brother of the late Professor Turner. He has not confined himself to the German abstract style, but has really presented us with an English book. After a careful perusal of its contents we can, with the exception of a few isolated points, confidently recommend it to the careful study of the student of medicine, which is more than we can say of the huge medical and surgical dictionaries that are too frequently obtruding themselves on our attention. The latter we cannot characterise better than as unwieldy documents, containing something that we wish to know, and everything that we do not wish to be acquainted with. We consider them exceedingly pernicious to students in consequence of the vagueness and old-wife nature of their details.

With regard to the other two works on our list we need only say that they are indispensable to the student of chemistry.

The progress which has been made in vegetable chemistry during the last twenty years, we venture to affirm, has been more rapid and astonishing than in the case of any other science in the course of a century. Twenty years ago, it is true, chemists were acquainted with a great many vegetable substances, and they knew, moreover, that these consisted of gases; but as to the position of the elements with reference to each other in these bodies, no proofs could be obtained. Oxalic acid, for example, was found to afford by analysis two atoms of carbon, and three atoms of oxygen, with one of hydrogen; and it was suspected that these elements might be thus arranged,—$CO + CO_2 + H$, or one atom carbonic oxide with one atom carbonic acid + one of hydrogen; but no proof could be afforded of this suspicion. But it is now demonstrated that such is its constitution, abstracting the hydrogen; for if we pour sulphuric acid upon crystals of oxalic acid in a retort, and distil over mercury, we obtain a quantity of gas, which, upon examination, turns out to be equal measures of carbonic oxide and carbonic acid, sufficient to account for the loss sustained by the matter in the retort. The hydrogen was found to belong to the water with which oxalic acid must be combined to enable it to exist in the solid state. From this view of the composition of oxalic acid, which is borne out by a great many other similar facts, it might be surmised that oxalic acid is a carbonate of the oxide of carbon. Carbonic oxide would, therefore, be a radical, forming chloro-carbonic acid, or chloride of oxide of carbon, when it combines with chlorine and oxamide, when two of its atoms unite with an atom of ammonia. From the recent researches of Liebig it appears highly probable that carbonic oxide enters as the basis of uric acid and a numerous suite of compounds which he has formed from that substance, in the form of the body *uril*, a compound of 2 atoms cyanogen + 4 carbonic oxide. The uril, therefore, is a *compound radical*, and, as all organic substances are obviously made up of such radicals, Liebig proposes to term organic chemistry the *chemistry of compound radicals*, and the inorganic branch of the science the *chemistry of simple radicals*. This definition is the more necessary because every day's experience is rapidly breaking down the barrier which formerly separated inorganic from organic substances. How can a body be said to be organic if it can be entirely constructed from inorganic elements by artificial means? Urea is in this predicament. The grand feat of producing an organic substance, or, rather, a *compound radical*, in the laboratory, was achieved by Wöhler, by passing dry ammoniacal gas and the vapour of hydrous cyanic acid into a dry vessel, and heating the woolly crystalline compound resulting from their union. Hence we learn that it will be by studying intimately the compound radicals and their affinities which are produced in the different processes in the human body, that we may expect a solution of digestion, secretion, &c.,—phenomena that still appear to many persons who will not examine them in their true light, as far beyond mortal ken. The futility of this notion is rendered apparent by the recent experiments of Dr. R. D. Thomson and Mr. Richardson, the translator of the second work on our list, (read at the British Association)* on vegetable albumen; in which they have shown that it is an *amide*; that is, a compound radical corre-

* Published in "Annalen der Pharmacie," for February, 1839.

sponding to a salt of ammonia deprived of an atom of water. Now, the elements of vegetable are almost identical with those of animal albumen. Hence there is a great step gained in physiology. No one knew what albumen was before. How was it possible, therefore, to say how starch could be converted into albumen, which is one of the most remarkable features of digestion?

Basyles.—The object, then, in organic chemistry is to arrange and simplify the study of the innumerable compounds which are presented to our view. The radicals already discovered which form acids are: *Carbonic oxide, cyanogen, mellon, benzyle, cinnamyle, salicyle, acetyle, formyle* and *kinoyle*; the known radicals forming bases are, *ethyle* and *methyle*. And here we must notice an error committed by the editor. Professor Graham proposes to term the ultimate radical, if we may so express it, the *basyle*. Thus, sodium is the *basyle* of sulphate of soda; soda is the *base*, and sulphatoxygen the *salt radical*, if we view this salt as consisting of $S O_4 + Na$, or sulphatoxide of sodium. Professor Liebig terms *ethyle*, ethule; *salicyle*, salicule, &c.; and his editor follows him: but this is wrong, because the Greek *v* is always *y* in English.

We have thus, as it were, an imperfect skeleton of an arrangement which will be rendered more and more complete, in proportion to the researches of chemists. But it is of a much more important benefit to labourers in this field, for it actually predicts where new substances are to be discovered. Chemistry is, therefore, becoming a science in some departments. To give an example: Oil of bitter almonds, when exposed to the air, becomes converted into benzoic acid; experiment showed that the acid differed from the oil, in containing an atom of hydrogen less, and an atom of oxygen more. Thus, oil of bitter almonds is the basyle of benzoic acid; the oil has, therefore, been called benzyle. Analogy leads to the induction that all acids similar to benzoic acid, will possess a basyle, and as far as research has proceeded, the result has been successful. There would also be inferred a presumption in favour of the nature of volatile oils analogous to the oil of bitter almonds.

We wish we could induce medical men to observe the steps of the chemist in generalising. He merely takes one or two facts which seem to lead to a generalisation, as an inducement or stimulus to determine how many cases are referrible to the apparent law which he is investigating. There is far more chemistry in medicine,—there is far more of exact science in medicine, than physicians are inclined to believe; and they will never appreciate the important fact until they have convinced themselves that it is absurd to go on from generation to generation prescribing herbs and plants, and fruitful trees, without rhyme or reason, only because their fathers did so before them; and until they set themselves in right earnest to study physiology through all its intricacies, of anatomy, chemistry, and physics, with pathology, or the departure from the natural structure and functions, and the action of remedies which shall enable us to restore the organs or functions to their natural state.

An interesting class of compound radicals is that of *amides* or *amidets*. When we distil oxalate of ammonia, or $C O, C O_2 + N H_3$, we obtain in the receiver a crystalline body, whose composition is $C_2 O_2 + N H_2$. It is, therefore, oxalate of ammonia deprived of an atom of water, but is quite a distinct substance, and is termed oxamide. This is the type of a number of *amides*. A striking character of this class of bodies is, that when boiled with potash they give out ammonia, and a salt of potash is formed. Thus, with oxamide oxalate of potash is the product of this treatment.

From this fact, which we owe to Dumas, it has been inferred that another compound of nitrogen and hydrogen exists, containing one atom less hydrogen—as represented in the preceding formula—$N H_2$, which it has been proposed to term amidogen. Ammonia will, therefore, be a hy*dret* of amidogen. Now, all the salts of ammonia contain water, which is necessary to their constitution; sulphate of ammonia, for example, or $N H_3 H O, S O_3$, may be viewed either as a sulphate of ammonia with water, or a sulphate of the oxide of *ammonium* or *ammonyle*, $N H_4 O S O_3$, where we have a new basyle; or if we consider the basyle similar to ethyle, as analogy would lead us to infer, we should have this substance a sulphatoxide of ammonyle, $N H_4 S O_4$, and, therefore, a compound similar to Glauber salt, or sulphatoxide of sodium, $Na S O_4$; and this leads us to the theory of

Ethyle or *ethers.*—This theory was first

proposed by Dr. Kane of Dublin, and Berzelius, and has been since strongly supported by Liebig of Giessen. The composition of sulphuric ether, as it is commonly termed, although when pure it contains no sulphuric acid whatever, has been found to be, 4 atoms carbon, 5 atoms hydrogen, and 1 atom oxygen. Now, according to the theory which we are considering, it is supposed that a substance exists although it has never been isolated, which has been termed *ethyle* (erroneously translated *ethule* by Dr. Turner), consisting of $C_4 H_5$. Ether is the oxide of ethyle $C_4 H_5 O$. Alcohol, from which ether is derived by the abstraction of water through the agency of sulphuric acid, is ether or the oxide of ethyle, with an atom of water; that is, in other words, a hydrate of the oxide of ethyle $C_4 H_5 O + H O$. We thus obtain a base for all the ethers. Nitric ether, for example, is a nitrate of the oxide of ethyle. But simple as this view of the question may appear, it should never be forgotten that it is a mere hypothesis recommended solely by its plausibility, because the base itself has never been separated; its isolation has foiled every attempt which has been hitherto made.

The number of Dr. Turner before us does not enter into the detail of these theories, but these will be found to be well explained in the pages of Thomson.

Before drawing this review to a conclusion, we propose making some observations more particularly on the translation of Liebig.

Hydrocyanic acid, p. 767.—The process of Clark for preparing this acid by means of tartaric acid and cyanide of potassium, is given, but there is surely some error. "According to Clark it may be prepared by dissolving 1 part of tartaric acid in 40 parts of water, and adding $2\frac{3}{4}$ parts of pure cyanide of potassium." The process of Clark is to dissolve $8\frac{1}{4}$ drs. of cyanide in 100 drs. of water, and add to the solution $18\frac{3}{4}$ drs. of tartaric acid previously dissolved in 20 drs. of water. The fluid will contain in solution $3\frac{3}{4}$ drs. of hydrocyanic acid, or 2.81 per cent. of the solution is hydrocyanic acid. Or a still better formula is, cyanide of potassium 1 oz., tartaric acid 2 oz., 2 drs., 1 scruple; distilled water 16 oz., 4 drs. The latter formula affords 1 grain of absolute hydrocyanic acid in 60 minims of medicinal solution. We are convinced that this is the cheapest and most certain method of forming prussic acid, and that when thus prepared, it is the most useful and certain remedy for nervous diseases of any in the pharmacopœia of all countries. In nervous headachs it gives immediate though not always permanent relief, and from the greatest agony the patient is generally restored for a longer or shorter time to perfect deprivation of pain. He who refuses to administer this medicine, therefore, when he knows it will relieve human suffering, is unworthy the place he holds in the medical profession. To prepare prussic according to the formula given above, it is necessary that the cyanide of potassium should be perfectly pure. The only specimens in the market that we have found pure were those manufacturing by Mr. Laming. Liebig states that the cyanide may be formed by adding hydrocyanic acid to a recently prepared concentrated solution of caustic potash, and evaporating the solution. We have great doubts about the success of this method of proceeding. At least, we have often tried, and have always obtained a mere solution of hydrocyanate of potash, and it has been reported to us by others that their attempts met with a similar result.

Murexid.—This substance was termed purpurate of ammonia by Dr. Prout, who discovered it. It may be formed by dissolving uric acid in dilute nitric acid and evaporating the solution till it acquires a flesh-red colour, when it is allowed to cool to 160°, and is then treated with dilute caustic ammonia in slight excess; the solution is then diluted with half its volume of boiling water, and allowed to cool; green crystals separate; these are a distinct substance, and not purpurate of ammonia.

Murexan, the *purpuric* acid of Prout.—It is prepared by dissolving murexid in caustic potash by the assistance of heat, which is applied till the blue colour disappears, when dilute sulphuric acid is added in excess. It falls in crystalline scales of a silky lustre. It is an *amide*, and not an acid, as Dr. Prout had inferred.

In conclusion we recommend the works under notice to the careful study of every medical man who wishes to rest his professional knowledge upon a philosophical basis.

ROYAL MEDICO-CHIRURGICAL SOCIETY.

Tuesday, April the 23rd, 1839.

Sir B. C. BRODIE, President.

CASE OF CONGENITAL ABSENCE OF THE PERICARDIUM.

By T. B. CURLING, *Esq., Assistant-Surgeon to the London Hospital, &c.*

THE subject of this observation was a gardener, aged 46, who, after exposure to wet, was admitted to the London Hospital in January, 1839, affected with paralysis, commencing in the lower extremities, and died in about three weeks. After death, besides some other interesting and rare morbid appearances, on raising the sternum with the costal cartilages, the heart was seen lying loose in the cavity of the left pleura in immediate contact with the left lung, without any appearance whatever of pericardial sac; it was rather large and flabby, and in its natural position. There was a small, white, opaque spot on the left ventricle, and also another on the left lung, where it was most exposed to the heart's motion. The author describes the relation of the left pleura to the great vessels and to the right pleura, and the deviation from its usual course of the left phrenic nerve, which he considers to be a discriminative mark of congenital deficiency of the pericardium from that which may ensue from diseases. The only semblance of pericardial sac that could be discovered was a pouch of serous membrane, above the diaphragm, at the point where the lower vena cava joins the heart. The author compares this case with those which he considers genuine similar cases recorded by other writers, and concludes with some remarks on the beneficial effects of a pericardial sac in isolating the heart, and thereby protecting it from the communication of inflammatory affections so frequently occurring in the lung.

Dr. BURNE inquired whether the patient in the case related suffered any inconvenience from change of position?

Mr. CURLING said, that as there had been no suspicion of heart disease until after death, the attention was not directed to that organ; he could not, therefore, answer the question satisfactorily.

Dr. BURNE remarked, that great difference of opinion appeared to exist amongst authors regarding the exact nature and situation of the white patches observed in this, and other cases, on the serous coat of the heart. Some had considered they were situated on the surface, whilst others believed they existed beneath the membrane. The appearance in question was very common; he had seen these spots in a great variety of states; sometimes half-organised, loose, and unwhitened; at other times firm and white. He did not think, with Dr. Hodgkin, that they were the result of friction; but he believed that they depended upon circumscribed local inflammation, throwing out fibrine, which became organised, and that they were situated on the free surface of the serous coat.

Remarks upon an Instrument for the Cure of Club-Foot, with Description of a Case of Congenital Varus, cured at the age of 23 years, by dividing the Tendo Achillis, the Tendon of the Abductor Pollicis Pedis, and the Plantar Fascia. By Dr. G. KRAUSS.

The author's object in this paper is chiefly to impress upon the minds of surgeons operating for the relief of the different forms of club-foot, that the success of the treatment, particularly in aggravated cases, depends mainly upon the perfection of the instrumental means employed for maintaining the position or form of the foot after the division of the tendons shall have been effected. With this view he gives, at some length, the details of a case of great deformity, as specified, and treated as detailed, in December 1837. Casts and instruments were also exhibited.

Sir B. BRODIE said that the operation for dividing tendons might be considered as an era in surgery, not only with regard to its importance as a means of cure in deformities of the feet, but also in its being applicable in a variety of cases. Mr. Liston had informed him that he had divided the ham-strings, in an adult person, whose knee had become contracted without disease of the joint. He (Sir Benjamin) had resorted to the division of tendons in several cases of contracted neck. The members were aware that there were two kinds of this deformity, one spasmodic, the other permanent. The operation in the first kind of cases was, as far as his experience went, of no benefit. Sir A. Cooper had once, against his inclination, but at the urgent request of the patient, and at the suggestion of Mr. Cline, divided the tendons of the sterno-mastoid muscles in a contraction of this description, but the operation was attended with no beneficial results. In cases of permanent contraction of the neck, the results of operative procedure were of great advantage. He (Sir Benjamin) had performed this operation as long ago as sixteen years. The patient was a young woman 18 or 20 years of age, who had suffered from the deformity for several years. He divided both the tendons, and kept the head in a proper position: the case was successful. He had since followed the same plan of treatment in several cases with a beneficial result. In one instance, in addition to contraction of the sterno-mastoid muscle, the edge of the trapezius was contracted through the

entire length of the muscle, the contracted border consisting of a rigid chord. In this case, after cutting through the tendons of the sterno-mastoid, he divided the constricted edges of the trapezius in several places, by introducing a small instrument, resembling a couching-needle (which he always used in this kind of operations); he afterwards kept the head in a proper position, and the deformity was removed, the chord, on his last examination of the patient, having nearly disappeared. He had also divided the palmar fascia in a gentleman, in whom the fingers were so much contracted that the points of them nearly met the palm of the hand. The deformity depended upon two or three hard contracted chords, formed of the palmar fascia, and the adhesion of this fascia, in several places, to the integuments. In this instance he divided the fascia in two or three different places; the hand was immediately easily extended, and having been kept in a splint for some time, became a good and serviceable member. He might mention, however, that in this case the pain from the operation was so excessive, that the patient, he believed, would not submit to another such operation, notwithstanding its success. Regarding the division of the ham-strings, in cases of contraction of the knee-joint, he had never found this proceeding necessary when the deformity arose from long-continued scrofulous disease in children, as in these instances he had observed that the mere application of mechanical apparatus was sufficient.

Mr. Benjamin Phillips said, that he intended shortly to lay before the Society the description of a case of contracted knee, in which he had divided the ham-strings. The patient was a young woman, twenty-five years of age; the contraction had been produced by rheumatic disease, and had resisted every kind of treatment and mechanical means of cure; the knee-joint was bent nearly to a right angle; there was no disease of the joint; flexion could be so far performed as to bring the heel in contact with the buttock, but extension was prevented from the contracted state of the chords in the ham. He first cut through the biceps on one side, and the semi-tendinosus on the other, and the knee could be immediately extended an inch and a half further than it could be previous to the operation. Finding, however, that further extension than this could not be produced, he divided the tendon of the semi-membranosus, without injury to the neighbouring parts; the limb became immediately extended three inches further, and by means of the screw apparatus the cure was rendered complete, the patient, at the expiration of eight or nine weeks, being able to walk about.

Mr. G. Smith, after division of the tendo Achillis, waited a week before he applied any instrument for the purpose of extending the foot. He thought this a more desirable mode than making any attempt at extension immediately after the operation.

Dr. Krauss generally commenced extension from the third day after the operation, and somewhat later in simple cases of pes equinus, where it was to be supposed that the extension of the foot would meet with no difficulty. He objected to immediate extension, because he thought it might produce irritation and inflammation of the wound. In simple cases, the fourth or fifth day was sufficiently early; and in more complicated ones, in which the cure would require months in its accomplishment, it was of no consequence whether the extending process was commenced two or three days sooner or later. He had, in several cases, divided the ham-strings in contraction of the knee. Great difficulty was often experienced in accomplishing the cure, particularly in cases of anchylosis which were the result of white swelling. The manner in which extension was brought about in these cases was of the utmost importance; the extension had, till now, been generally effected by using the leg and thigh as levers, but the whole process in this mode acted back upon the knee-joint. In the first case of anchylosis of the knee, in which he divided the tendons, inflammation of rather a serious character took place in the joint. This led him to the invention of an apparatus which effected extension not only without irritation to the knee-joint, but even by diminishing the pain that existed in the knee. Stromeyer, in his recent publication, had given an interesting case of spasmodic contraction of the neck, in which the sternal portion of the sterno-cleido-mastoideus was the seat of the spasm. After the tendon of this portion of the muscle was divided, the spasm attacked the clavicular part of the muscle; this tendon was also divided, and the spasms removed; they, however, attacked a portion of the trapezius, but on its division by the knife the spasms were entirely subdued.

Sir B. Brodie inquired how long Stromeyer's patient had remained well after division of the sterno-mastoid muscle; he (Sir Benjamin) could not conceive how such an operation could be beneficial in cases of wry neck resulting from spasmodic action, connected as these cases always were with disease of the nervous system. Spasmodic contraction of the neck might go away entirely for a long period and return as bad as ever; he knew an instance of a lady who was alternately affected, for twelve months at a time, with insanity and contracted neck. Regarding extension after division of the tendo Achillis, he had usually enforced it in some slight degree immediately after the operation, and he had never found any ill

results from it. M. Guedrin had, however, informed him that it was much more advisable to postpone the application of any instrument for the purpose of extending the foot, until the expiration of three weeks from the operation. M. Guedrin's opinion deserved attention.

Dr. KRAUSS said that Dr. Stromeyer's patient had remained well at the end of a year from the date of the operation. He (Dr. K.) had, in a case of spasmodic contraction of the muscles of the calf, divided the tendo Achillis twelve months since, and up to the present time it had been successful.

MEDICAL SOCIETY OF LONDON.

Monday, May 6th, 1839.

BELLADONNA IN ERYSIPELAS.

AT the last meeting the members were occupied with a discussion on the treatment of erysipelas.

Mr. HEADLAND made some remarks on the great variety of treatment recommended by various teachers in this disease, a fact which he thought an opprobrium to the science of medicine. He had read some cases of erysipelas reported in THE LANCET, in which Mr. Liston had employed the extract of belladonna, in small doses, with great effect. He (Mr. Headland) had since tried this remedy in three cases, and the effects were most satisfactory,—more satisfactory, indeed, than those from any other remedies which he had ever employed. From the good effect of belladonna in scarlatina, as well as in the disease in question, he believed that it possessed a specific action upon the skin.

APPROACHING

ELECTION OF A CORONER AT BIRMINGHAM.

ARGUMENTS IN FAVOUR OF A MEDICAL CORONER.

THE "Birmingham Journal" of May 4, contains a "leading article" on this subject, from which we abstract the following statement, and the sound and forcible arguments which attend it. We are rejoiced to find that the opinion recently pronounced by the freeholders of Middlesex in favour of medical Coroners is producing the agitation of this important question in other parts of the kingdom wherever opportunities occur for its consideration. Though the first attempt to reform the barbarous custom of appointing men who are ignorant of medicine to preside at inquests will not *at once* be successful in every place, yet no man can estimate justly the effects of discussion without feeling assured that if medical practitioners be equally true to themselves, and considerate of the public welfare, the next trials that arise must find the more enlightened community demanding, with irresistible force, the election of Coroners from amongst the members of our profession. We should entertain a confident hope of a wise result at Birmingham, at the approaching election, if we could feel that medical men generally were knit together by as strong a bond of union, and animated by as fervent an *esprit du corps*, as the attorneys of the kingdom.

In giving this long extract from the Journal we are satisfying our desire to supply to our professional brethren every argument that comes before us, whether in a new or an old shape, which may prove, on future occasions, to be of use to them as advocates of medical Coroners, or candidates for the office.

Mr. Alderman BEALE last week gave the following notice of motion for the monthly meeting of the Council of the Corporation of Birmingham, on Tuesday next,—"That he shall propose for the decision of the Council, with reference to the appointment of Coroner, the expediency of choosing a member of the legal or the medical profession for that office." There were five candidates for the office; three attorneys and two members of the medical profession.

The propriety of the Council's determining, *in the first instance*, from *which* of the two professions the election should be made, has been the subject of much conversation in Birmingham. Without question, the Council may pass a resolution in favour of a medical Coroner, or in a favour of a legal Coroner; and the same majority may give a corroborative vote in favour of either gentleman, as the case may be. But, we must deny their power, by any preliminary vote, so to circumscribe the rights of its members, as to prevent them from afterwards voting for any person, legal or medical. We do not, of course, mean that the members of Council may not come to a mutual understanding, that if the majority approve of a medical Coroner, only a medical Coroner shall be nominated. Neither would we object to the expediency of such a preliminary resolution. If it tend to relieve the councillors from the operation of any *personal feelings* that might interfere with a judicious choice, we recommend it.

The question—whether a legal or medical Coroner is likely better to fulfil the important duties of the office, we raised some time ago; and the notice of Mr. Beale offers us an opportunity of again recurring to it. We shall give first the following "Few plain reasons" on the subject, with which a correspondent has furnished us:—

1. More or less of medical knowledge is required at *almost every* inquest. Medical knowledge of an elaborate kind is indispensable at *many*.

An inquest is an inquiry "upon view" as to the cause of death. This is sought by the instrumentality of one learned in the causes of death, and their vestiges; by the opinion of that witness the jury ought to be governed. But to *appreciate his testimony* requires medical "skill" in the court,—to separate what is scientific certainty in the opinion, from crude dicta,—to demonstrate to plain men the precise point where this testimony is really "evidence," and where it falls short of it. This requires in the court a sound acquaintance with science, and the complements which new researches are daily adding to them. It is impossible for an attorney to do this. Is the case one of poisoning? The quantity left for examination may be minute—the state of admixture complex—the agent used unknown. The legal Coroner's interrogatory is necessarily limited to—"Did you analyse the materials? Did you satisfy yourself of the absence or presence of poison?" There he stops. It is impossible for him to go any further without exposing himself. Hence guilt often escapes, and the innocent is subjected to suspicion; the verdict must inevitably partake of the nature of hazard, and the ends contemplated by the institution fail, by the defect of the presiding officer. In the higher courts the case is different. Adverse medical and legal talents may there be adduced, by the parties, to any extent that is requisite to substantiate the truth; but in a tribunal of a preliminary kind, the person presiding having the care of the king's interest, as well as the prisoner's, indispensably requires an ample knowledge of the subjects upon which all sorts of medical witnesses (good, bad, and indifferent) are summoned. The records of forensic medicine are replete with instructive instances resulting from the want of it. Of the mineral poison, which is in most common use, and for detecting which the processes have been more studied than any other,—arsenic,—it may be roundly stated that very few, if any, of the legal Coroners of the kingdom, are competent to observe upon the proof of its detection, by the employment of the resources of modern chemistry. In cases of mechanical injury, "wounds and blows" form a large subject of study. How can the solicitor say whether the medical witness's opinion is correct or not, or is in accordance with the facts of science, when he pronounces a bruise, cut, wound, burn, scald, or extravasation of blood to have taken place *before death or after it*?—that a red spot upon a knife or garment was the result of *blood, rust, lemon-juice, or madder*?—that a gun or pistol had been discharged within certain and definite limits of time?—that a large aperture in the stomach was produced by spontaneous disease, and itself the cause of death—followed death—(being the result of a solvent power of the gastric juice)—or took place (either before or after) through the agency of a corrosive poison introduced to excite suspicions? All these questions most commonly admit of a satisfactory solution. But the hardihood of a dogmatic medical witness is often more eminent than his knowledge of the facts by which these problems are resolved. Upon his answers the verdict turns; and, if the Coroner cannot develop the value of the proof he gives, the object of the court is frustrated. In cases of infanticide, hardly an inquest is held that a legal Coroner does not tell the jury that the hydrostatic test (the proof that the child has been born alive, because the lungs float in water) is now "utterly exploded!"

2. The cases of inquest are few which demand any acquaintance with law, beyond what is supplied by the ordinary forms and precedents. The extent of legal knowledge ever required is very limited. It chiefly relates to one single topic, homicide, and, in acquiring it, there is not the slightest difficulty.

To show how little legal knowledge is required by the Coroner, it may be stated that the whole of "the office and duties of the Coroner's Court," by Jervis, only extends to 294 small pages. The same subjects are comprised in another book of the same kind, Grindon's "Lex Coronatoria, or the Law and Practice of the Coroner's Court," in 207 pages. And all the important legal part of either, viz., that which relates to the several forms of *homicide*, is better disposed of in 24 pages of "Burns' Justice." Everything relative to deodand, flight, and forfeiture, or proceedings upon the inquest, literally, does not require to be twice perused by any one of average intellect; and, for framing the record, there is an abundance of forms and precedents appended to each of the first-named books. A delusive argument has been put forth, on more than one occasion, namely, that the collateral circumstantial evidence renders it necessary that the Coroner should be a lawyer. It may well be questioned how far an attorney has any advantage in educing veritable inferences from minute facts. But, in addition to the circumstantial moral evidence, the Coroner has the aid of the evidence from *post-mortem* appearances, together with the *chemical* evidence; whereas, if the homicide be more difficult of elucidation, then any plain justice of the peace is presumed to be lawyer enough to be entrusted with this investigation; and not this only, but with the circumstantial proof of all treasons, felonies, misdemeanours, and crimes.

3. The education, the habits, and the modes of thinking, to which the medical

man is used, *cæteris paribus*, form a better preparation than those of the lawyer.

The medical man engaged in practice is wont, every hour, to probe how far his patients are deceiving him or themselves,—to search, sift, and strike a balance between the weight of conflicting facts. His sole and exclusive object is to elicit the reality with a view to practical results. On the other hand, the lawyer's aim is to *make a side*; his search for truth is, at most, but incidental. It does not suit the nature of his business to court nothing but the truth, nor to scan his client's allegations with a philosophy, fearless, exact, and scrupulous. It cannot be doubted but that special pleading is a bad school for learning the investigation of truth.

4. One conversant with the symptoms of disease cannot fail to be the fitter and more competent judge whether the "dying suddenly" is such as really to call for the holding of an inquest, or not, a discretion by law residing entirely in the Coroner's breast.

5. None but a medical man can duly appreciate and charge the jury as to the effect and value of the testimony of the chief witness adduced.

6. The results of experience, for some years past, are in favour of the choice of a medical man, and, as all the medical authorities now require an express cultivation of these subjects, it is probable that the difference will become still more marked.

Two inquests, taken by attorney Coroners, have been quashed in the Court of Queen's Bench, since the Middlesex election for Coroner in 1831. But, during that period, there is no tangible proof of any complaint against an inquisition taken by a medical Coroner. Of medical Coroners there are upwards of twenty-five, mostly, if not all, with the exception of Mr. Wakley, engaged in practice; and the beneficial effects of the appointment of one thoroughly possessed of the peculiar requisite attainments, has seldom been more forcibly demonstrated than in the election of the hon. Member for Finsbury, by the Middlesex freeholders. The knowledge of the fact of his election is already operating most beneficially upon some of the metropolitan institutions.

These reasons are so very plain, and withal so convincing, says the editor of the "Birmingham Journal," that we scruple to weaken their power by adding to them. We shall merely indicate two arguments, which our correspondent has not touched on.

First,—Supposing a legal Coroner to possess the medical knowledge that is necessary to fit him for the proper examination of a medical witness, such a knowledge would not be so available in his hands as in the hands of a medical Coroner. The great check against rash and ignorant assertion in a medical or any other witness, is the certainty that it will meet with instant correction from the judge, *and that the public will go along with the judge*. Now, supposing a lawyer to convict a medical witness of the grossest ignorance, the dictum of a lawyer on such a subject has no weight, and the witness knows it. But let a medical witness, intentionally or carelessly, make a slip, while under examination by a medical judge, and be convicted of such a slip by a medical judge, and the witness loses reputation, as a necessary consequence. Not only, therefore, is a medical Coroner better qualified to elicit the truth from a medical witness, but a medical witness will be always best prepared, and most ready to tell the truth to a medical Coroner.

Secondly,—There is this point of importance in coming to a conclusion on this question. The judgment of an inquest jury is twofold. The preliminary, and, by far the most important, inquiry for the jury is, whether the death was in the course of nature or not. *This is an inquiry purely medical.* Upon its judicious management will depend whether very great injury may not be caused to society by the diffusion of needless terror and alarm, and very great wrong inflicted upon an individual, by the imputation of a heinous crime, where no crime at all may have been committed. Such an inquiry, when the case is at all complicated, can hardly be judiciously or satisfactorily managed, unless by a medical man.

The tracing of the agent of the death, even supposing the death not to have arisen from natural causes, is a matter of legal evidence, and its classification a matter of law. But, though this part of the inquest is not without its grave difficulties, let it be remembered, that a mistake here is far more venial than in the former case. It is a very different matter for a man to be accused, on imperfectly understood evidence, of a crime which has been actually committed, and to be accused where no crime has been committed, and where no guilt can, by possibility, attach. In the one case there is an offence against society, which justifies even a surplusage of caution, in the other there is no offence, and, therefore, no fear, and no demand for extraordinary means of safeguard.

From these considerations, as well as from the plain reasons to which we offer them as a supplement, we are of opinion that a medical man is better fitted for the due fulfilment of the duties of the office of Coroner, than a lawyer. We come to this conclusion from no personal feelings, and, therefore, shall not even hint at the positive, much less the comparative, merits of the several candidates, all of whom we believe to be most worthy and honourable persons.

IMPERFECT INQUESTS.

To the Editor of THE LANCET.

SIR:—The following statement of the mode of conducting inquisitions in this country requires to be explained and better understood, not only by the medical profession, but by every individual who is tenacious of his own life, or that of any member of his family.

I was called to see a man on Tuesday last, at one, P.M., who was found dead in his bed; from the appearance of the body he had been dead some six or eight hours; there were not any indications about the body more than are usual with persons dying a natural death, and having made the usual examination I came to that conclusion: in this instance, however, on examining the room with two other persons, we found in a leather hatbox some loose papers, also some carbonate of soda and tartaric acid, in separate parcels, and likewise a half-ounce phial, in which had been hydrocyanic acid. Now, Sir, the following is the subject matter to which I wish more particularly to draw your attention:—An inquest was held on the following day; I was not subpœnaed, but as one of the relatives who was very desirous that my deposition should be had, asked the beadle if he had summoned the surgeon, the former replied, "Oh, no, we don't summon doctors now, unless in bad cases, for they get a guinea *out of us*,"—the brother of the deceased immediately came to my house, and requested me to be in attendance, if called on by the Coroner or jury. I did so, but no message arrived; the inquest went on, and a verdict of *natural death* returned. On my seeing the witnesses subsequently, they were pleased to relate their depositions, and one of them was asked by the jury if there were any loose phials found about the room, to which he replied, none. I directly put this question to him,—"Did you not say a word about the bottle which I have?" And mark his answer,—"That was not a *loose* bottle, for it was in the hatbox." Now, Sir, this said phial is still in my possession; and, although from what I saw of the body *externally*, I am of opinion that the verdict was a correct one, yet if any inquiry whatever be necessary, it is disgraceful that it is not done with more accuracy and justice. I trust this description of the present system will induce every man to use his best endeavours to correct it. I am, Sir, yours obediently,

M.R.C.S.

May 1, 1839.

P. S. Since writing the above the proprietor of the house has given me another phial, with a full and large label on it, prussic acid, likewise the word poison, which was found beneath the bedclothes, on the body being moved into the shell, and which has some of the acid remaining in it. He says that he mentioned at the inquest that the medical man was at home if they wished to see him, and they dissented, no doubt, Sir, to save the county of Middlesex the fee of twenty-one shillings. From the circumstance of the second bottle being discovered, perhaps some of my professional brethren may doubt my penetration in not discovering the smell of prussic acid on the deceased, but I can assure them that both myself and another medical man were particularly desirous of doing so, but not a vestige was to be found by the sense of smelling, or any other indication whatever.

THE NEW PHARMACOPŒIA.

To the Editor of THE LANCET.

SIR:—I beg to call your attention to one among many unaccountable alterations in the new Pharmacopœia, which is calculated to mislead unsuspecting practitioners,—practitioners, I mean, who are not disposed to attribute to the Royal College of Physicians, as a body, infirmities or levities which do not infect persons of no pretensions to superior discernment or superior gravity.

I have been for some time in the habit of administering the liquor arsenicalis in chronic diseases, with much care and some success, and had just arrived at the conclusion that it had generally been exhibited in doses too large, and for an inadequate length of time, when I was a little baffled in my inquiries by a phial being returned on my hands, with a message from the lady to whom it was sent, to the effect that some mistake had been made, that the medicine tasted *stronger*, and smelt more of scent than usual. Upon inquiry, I found that the contents of the phial had been mixed from a bottle of the liquor arsenicalis recently arrived from Apothecaries' Hall, the appearance of which led me to examine the formula in the new Pharmacopœia,—when, to my surprise and consternation, I found that *eighty grains* of arsenic were substituted for *sixty-five*, and five fluid drachms of the tincture of lavender were substituted for four fluid drachms. My respect for the College of course prevented my throwing the blame upon that learned body. But my patient looked very incredulous when I protested that there was "no mistake," and I think I read in her looks, also, that she had made up her mind never to put any faith in medicine sent from my shop for the future, as we were careless even when handling arsenic, which she was aware she was taking.

Upon turning to the translations of the new Pharmacopœia by Mr. R. Phillips and Dr. Collier respectively, I find that neither of these gentlemen notice the alteration in the strength of this important solution.

Being in the region of poisons I turned to examine the fate of calomel and corrosive sublimate, and find that the new designation of the former of these dangerous medicines is hydrargyri chloridum, which Mr. R. Phillips thinks should be called the proto-chloride whenever "*a pointed distinction is necessary*;" and Dr. Collier will "be agreeably surprised if this omission does not make work for the Coroner."

My curiosity being still more excited by this discovery, I turned to the article acidum hydrocyanicum dilutum, and here I find a new strength prescribed as the standard of this most perilous medicine, and a strength which, to practitioners not well versed in practical chemistry, recommends itself by bearing no obvious or intelligible proportion to the strength of the formulæ in common use, thus rendering the preparation a thing having nothing to recommend it but its *uncertainty*. Uncertainty! where a single drop may destroy a patient!

We now turn to tincture of opium. Surely we are allowed a standard strength for this very useful and very common form for the administration of another dangerous poison. No such thing. Three ounces of opium are substituted for two ounces and a half; and either we waste four ounces of opium in every gallon of laudanum we make, or every *fifty minims* we use is equivalent to *sixty minims* of the old tincture!!

I had intended to pursue my researches; but I have already made as many discoveries as my memory can retain. All I have to beg is, that you will make this affair as public as you can, in order that every general practitioner may see the necessity of committing both the old and new Pharmacopœia to memory, which he must do, unless he wishes to be convicted of manslaughter at a coroner's inquest. I remain, Sir, yours faithfully,

A General Practitioner.

April 27, 1839.

THE FŒTUS FREE FROM THE OPERATION OF THE IMAGINATION.

A correspondent at Guildford writes as follows:—

About eight years ago I assisted at the removal of a supernumerary thumb from the left hand of an infant, which had been born with this additional member under the following circumstances:—On the occurrence of the birth of a former child inquiries were made of the husband by the lady's father regarding the health of his daughter, and received the satisfactory information that she was doing well, but that the child, "like all her children at birth, was little bigger than a thumb." The lady's father (an octogenarian) dining with friends that day, and in his dotage misapprehending the observation, stated to his guests, as a very singular circumstance, "that all his daughter's children had been born with two thumbs on the left hand;" the observation excited much merriment, and the father's story was next day jocularly detailed to the daughter. She again became pregnant, and her mind reverted to the tale of a child with two thumbs on the *left* hand; and, during the whole period of gestation, her mind was under the conviction that her child would be born with this deformity. Her first inquiry after its birth was, "Nurse, has not the child two thumbs on the left hand?" It had. Within a month from the time at which the above case occurred, the wife of a tradesman in my vicinity gave birth to an infant with an exactly similar deformity, two thumbs on the *left* hand; but, unlike the mother in the former case, she had never for a moment entertained a thought of any malformation being likely to befal her child. To my mind these two cases strikingly illustrate the fallacy of the opinion, that the mother's mind exerts any influence over the configuration of the fœtus in utero.

TO CORRESPONDENTS.

An Old Subscriber. Guy's, nearly 600; St. Thomas's, 450; St. Bartholomew's, 540.

The sort of controversy in which *A General Practitioner's Pupil*, *An Assistant*, and several other correspondents, offer to engage, experience is continually proving to be useless disputation. Reply follows allegation, rejoinder awaits reply, replication attends rejoinder, answers follow replications, and fresh replies await those answers in endless succession, all containing general assertions in place of specific facts, without satisfaction to any but the debaters. Nothing is gained by republishing the assertions of antagonists one week simply to have them denied in the next. Facts are welcome and conclusive. They may be difficult to obtain, but no reform is promoted without them.

Medico. Any slight, unsubstantial deviations would be overlooked by the Company, but they must not be of material importance, or be indicative, on the part of the candidate, of his having premeditated to act in disobedience of the prescribed forms.

The publication of the letter of *An Enemy to Humbug* would only serve as an advertisement for the quack alluded to.

The reports from St. Thomas's and the Westminster Hospitals, and the communications of *Dr. Farell*, *Mr. Pratt*, and *Mr. Maddock*, next week.

Errata.—In p. 218, col. 2, line 44, *instead of* "I," *substitute* and; p. 220, *for* "it is contagious," *read* it is not contagious.

Page 233, col. 1, line 21, *for* "fluid," *read* florid; line 47, *for* "fluid," *read* florid.